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OF THE

ROYAL SOCIETY

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FOR THE YEAR MDCCCIV.

PART I.

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MDCCCIV.

ADVERTISEMENT.

THE Committee appointed by the *Royal Society* to direct the publication of the *Philosophical Transactions*, take this opportunity to acquaint the Public, that it fully appears, as well from the council-books and journals of the Society, as from repeated declarations which have been made in several former *Transactions*, that the printing of them was always, from time to time, the single act of the respective Secretaries, till the Forty-seventh Volume: the Society, as a Body, never interesting themselves any further in their publication, than by occasionally recommending the revival of them to some of their Secretaries, when, from the particular circumstances of their affairs, the *Transactions* had happened for any length of time to be intermitted. And this seems principally to have been done with a view to satisfy the Public, that their usual meetings were then continued, for the improvement of knowledge, and benefit of mankind, the great ends of their first institution by the Royal Charters, and which they have ever since steadily pursued.

But the Society being of late years greatly enlarged, and their communications more numerous, it was thought advisable, that a Committee of their members should be appointed, to reconsider the papers read before them, and select out of them such as they should judge most proper for publication in the future *Transactions*; which was accordingly done upon the 26th of March, 1752. And the grounds

of their choice are, and will continue to be, the importance and singularity of the subjects, or the advantageous manner of treating them; without pretending to answer for the certainty of the facts, or propriety of the reasonings, contained in the several papers so published, which must still rest on the credit or judgment of their respective authors.

It is likewise necessary on this occasion to remark, that it is an established rule of the Society, to which they will always adhere, never to give their opinion, as a Body, upon any subject, either of Nature or Art, that comes before them. And therefore the thanks which are frequently proposed from the Chair, to be given to the authors of such papers as are read at their accustomed meetings, or to the persons through whose hands they receive them, are to be considered in no other light than as a matter of civility, in return for the respect shewn to the Society by those communications. The like also is to be said with regard to the several projects, inventions, and curiosities of various kinds, which are often exhibited to the Society; the authors whereof, or those who exhibit them, frequently take the liberty to report, and even to certify in the public news-papers, that they have met with the highest applause and approbation. And therefore it is hoped, that no regard will hereafter be paid to such reports and public notices; which in some instances have been too lightly credited, to the dishonour of the Society.

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THE PRESIDENT and COUNCIL of the ROYAL SOCIETY adjudged, for the Year 1803, the Medal on Sir GODFREY COPLEY's Donation, to RICHARD CHENEVIX, Esq. F. R. S. for his various chemical Papers, printed in the Philosophical Transactions.

PHILOSOPHICAL

TRANSACTIONS.

I. *The* Bakerian *Lecture. Experiments and Calculations relative to physical Optics. By* Thomas Young, *M. D. F. R. S.*

Read November 24, 1803.

I. EXPERIMENTAL DEMONSTRATION OF THE GENERAL LAW OF THE INTERFERENCE OF LIGHT.

In making some experiments on the fringes of colours accompanying shadows, I have found so simple and so demonstrative a proof of the general law of the interference of two portions of light, which I have already endeavoured to establish, that I think it right to lay before the Royal Society, a short statement of the facts which appear to me so decisive. The proposition on which I mean to insist at present, is simply this, that fringes of colours are produced by the interference of two portions of light; and I think it will not be denied by the most prejudiced, that the assertion is proved by the experiments I am about to relate, which may be repeated with great ease, whenever

the sun shines, and without any other apparatus than is at hand to every one.

Exper. 1. I made a small hole in a window-shutter, and covered it with a piece of thick paper, which I perforated with a fine needle. For greater convenience of observation, I placed a small looking glass without the window-shutter, in such a position as to reflect the sun's light, in a direction nearly horizontal, upon the opposite wall, and to cause the cone of diverging light to pass over a table, on which were several little screens of card-paper. I brought into the sunbeam a slip of card, about one-thirtieth of an inch in breadth, and observed its shadow, either on the wall, or on other cards held at different distances. Besides the fringes of colours on each side of the shadow, the shadow itself was divided by similar parallel fringes, of smaller dimensions, differing in number, according to the distance at which the shadow was observed, but leaving the middle of the shadow always white. Now these fringes were the joint effects of the portions of light passing on each side of the slip of card, and inflected, or rather diffracted, into the shadow. For, a little screen being placed a few inches from the card, so as to receive either edge of the shadow on its margin, all the fringes which had before been observed in the shadow on the wall immediately disappeared, although the light inflected on the other side was allowed to retain its course, and although this light must have undergone any modification that the proximity of the other edge of the slip of card might have been capable of occasioning. When the interposed screen was more remote from the narrow card, it was necessary to plunge it more deeply into the shadow, in order to extinguish the parallel lines; for here the light, diffracted from the edge of the object, had entered further into

the shadow, in its way towards the fringes. Nor was it for want of a sufficient intensity of light, that one of the two portions was incapable of producing the fringes alone; for, when they were both uninterrupted, the lines appeared, even if the intensity was reduced to one-tenth or one-twentieth.

Exper. 2. The crested fringes described by the ingenious and accurate GRIMALDI, afford an elegant variation of the preceding experiment, and an interesting example of a calculation grounded on it. When a shadow is formed by an object which has a rectangular termination, besides the usual external fringes, there are two or three alternations of colours, beginning from the line which bisects the angle, disposed on each side of it, in curves, which are convex towards the bisecting line, and which converge in some degree towards it, as they become more remote from the angular point. These fringes are also the joint effect of the light which is inflected directly towards the shadow, from each of the two outlines of the object. For, if a screen be placed within a few inches of the object, so as to receive only one of the edges of the shadow, the whole of the fringes disappear. If, on the contrary, the rectangular point of the screen be opposed to the point of the shadow, so as barely to receive the angle of the shadow on its extremity, the fringes will remain undisturbed.

II. COMPARISON OF MEASURES, DEDUCED FROM VARIOUS EXPERIMENTS.

If we now proceed to examine the dimensions of the fringes, under different circumstances, we may calculate the differences of the lengths of the paths described by the portions of light, which have thus been proved to be concerned in producing those

fringes; and we shall find, that where the lengths are equal, the light always remains white; but that, where either the brightest light, or the light of any given colour, disappears and reappears, a first, a second, or a third time, the differences of the lengths of the paths of the two portions are in arithmetical progression, as nearly as we can expect experiments of this kind to agree with each other. I shall compare, in this point of view, the measures deduced from several experiments of NEWTON, and from some of my own.

In the eighth and ninth observations of the third book of NEWTON'S Optics, some experiments are related, which, together with the third observation, will furnish us with the data necessary for the calculation. Two knives were placed, with their edges meeting at a very acute angle, in a beam of the sun's light, admitted through a small aperture; and the point of concourse of the two first dark lines bordering the shadows of the respective knives, was observed at various distances. The results of six observations are expressed in the first three lines of the first Table. On the supposition that the dark line is produced by the first interference of the light reflected from the edges of the knives, with the light passing in a straight line between them, we may assign, by calculating the difference of the two paths, the interval for the first disappearance of the brightest light, as it is expressed in the fourth line. The second Table contains the results of a similar calculation, from NEWTON'S observations on the shadow of a hair; and the third, from some experiments of my own, of the same nature: the second bright line being supposed to correspond to a double interval, the second dark line to a triple interval, and the succeeding lines to depend on a continuation of the progression. The unit of all the Tables is an inch.

Table I. *Obs.* 9. N.

Distance of the knives from the aperture						101.
Distances of the paper from the knives	$1\frac{1}{2}$,	$3\frac{1}{3}$,	$8\frac{3}{5}$,	32,	96,	131.
Distances between the edges of the knives, opposite to the point of concourse	.012,	.020,	.034,	.057,	.081,	.087.
Interval of disappearance	.0000122,	.0000155,	.0000182,	.0000167,	.0000166,	.0000166.

Table II. *Obs.* 3. N.

Breadth of the hair		$\frac{1}{280}$.
Distance of the hair from the aperture		144.
Distances of the scale from the aperture	150,	252.
(Breadths of the shadow	$\frac{1}{54}$,	$\frac{1}{9}$.)
Breadth between the second pair of bright lines	$\frac{2}{47}$,	$\frac{4}{17}$.
Interval of disappearance, or half the difference of the paths	.0000151,	.0000173.
Breadth between the third pair of bright lines	$\frac{4}{73}$,	$\frac{3}{10}$.
Interval of disappearance, $\frac{1}{4}$ of the difference	.0000130,	.0000143.

Table III. *Exper.* 3.

Breadth of the object	.434.
Distance of the object from the aperture	125.
Distance of the wall from the aperture	250.
Distance of the second pair of dark lines from each other	1.167.
Interval of disappearance, $\frac{1}{3}$ of the difference	.0000149.

Exper. 4.

Breadth of the wire	.083.
Distance of the wire from the aperture	32.
Distance of the wall from the aperture	250.
(Breadth of the shadow, by three measurements	.815, .826, or .827; mean, .823.)
Distance of the first pair of dark lines	1.165, 1.170, or 1.160; mean, 1.165.
Interval of disappearance	.0000194.
Distance of the second pair of dark lines	1.402, 1.395, or 1.400; mean, 1.399.
Interval of disappearance	.0000137.
Distance of the third pair of dark lines	1.594, 1.580, or 1.585; mean, 1.586.
Interval of disappearance	.0000128.

It appears, from five of the six observations of the first Table, in which the distance of the shadow was varied from about 3 inches to 11 feet, and the breadth of the fringes was increased in the ratio of 7 to 1, that the difference of the routes constituting the interval of disappearance, varied but one-eleventh at most; and that, in three out of the five, it agreed with the mean, either exactly, or within $\frac{1}{160}$ part. Hence we are warranted in inferring, that the interval appropriate to the extinction of the brightest light, is either accurately or very nearly constant.

But it may be inferred, from a comparison of all the other observations, that when the obliquity of the reflection is very great, some circumstance takes place, which causes the interval thus calculated to be somewhat greater: thus, in the eleventh line of the third Table, it comes out one-sixth greater than the mean of the five already mentioned. On the other hand, the mean of two of Newton's experiments and one of mine, is a result about one-fourth less than the former. With respect to the nature of this circumstance, I cannot at present form a decided opinion; but I conjecture that it is a deviation of some of the light concerned, from the rectilinear direction assigned to it, arising either from its natural diffraction, by which the magnitude of the shadow is also enlarged, or from some other unknown cause. If we imagined the shadow of the wire, and the fringes nearest it, to be so contracted that the motion of the light bounding the shadow might be rectilinear, we should thus make a sufficient compensation for this deviation; but it is difficult to point out what precise track of the light would cause it to require this correction.

The mean of the three experiments which appear to have been least affected by this unknown deviation, gives .0000127

for the interval appropriate to the disappearance of the brightest light; and it may be inferred, that if they had been wholly exempted from its effects, the measure would have been somewhat smaller. Now the analogous interval, deduced from the experiments of NEWTON on thin plates, is .0000112, which is about one-eighth less than the former result; and this appears to be a coincidence fully sufficient to authorise us to attribute these two classes of phenomena to the same cause. It is very easily shown, with respect to the colours of thin plates, that each kind of light disappears and reappears, where the differences of the routes of two of its portions are in arithmetical progression; and we have seen, that the same law may be in general inferred from the phenomena of diffracted light, even independently of the analogy.

The distribution of the colours is also so similar in both cases, as to point immediately to a similarity in the causes. In the thirteenth observation of the second part of the first book, NEWTON relates, that the interval of the glasses where the rings appeared in red light, was to the interval where they appeared in violet light, as 14 to 9; and, in the eleventh observation of the third book, that the distances between the fringes, under the same circumstances, were the 22d and 27th of an inch. Hence, deducting the breadth of the hair, and taking the squares, in order to find the relation of the difference of the routes, we have the proportion of 14 to $9\frac{1}{4}$, which scarcely differs from the proportion observed in the colours of the thin plate.

We may readily determine, from this general principle, the form of the crested fringes of GRIMALDI, already described; for it will appear that, under the circumstances of the experiment related, the points in which the differences of the lengths of the

paths described by the two portions of light are equal to a constant quantity, and in which, therefore, the same kinds of light ought to appear or disappear, are always found in equilateral hyperbolas, of which the axes coincide with the outlines of the shadow, and the asymptotes nearly with the diagonal line. Such, therefore, must be the direction of the fringes; and this conclusion agrees perfectly with the observation. But it must be remarked, that the parts near the outlines of the shadow, are so much shaded off, as to render the character of the curve somewhat less decidedly marked where it approaches to its axis. These fringes have a slight resemblance to the hyperbolic fringes observed by Newton; but the analogy is only distant.

III. Application to the Supernumerary Rainbows.

The repetitions of colours sometimes observed within the common rainbow, and described in the Philosophical Transactions, by Dr. Langwith and Mr. Daval, admit also a very easy and complete explanation from the same principles. Dr. Pemberton has attempted to point out an analogy between these colours and those of thin plates; but the irregular reflection from the posterior surface of the drop, to which alone he attributes the appearance, must be far too weak to produce any visible effects. In order to understand the phenomenon, we have only to attend to the two portions of light which are exhibited in the common diagrams explanatory of the rainbow, regularly reflected from the posterior surface of the drop, and crossing each other in various directions, till, at the angle of the greatest deviation, they coincide with each other, so as to produce, by the greater intensity of this redoubled light, the common rainbow of 41 degrees. Other parts of these two portions will quit the drop

in directions parallel to each other; and these would exhibit a continued diffusion of fainter light, for 25° within the bright termination which forms the rainbow, but for the general law of interference, which, as in other similar cases, divides the light into concentric rings; the magnitude of these rings depending on that of the drop, according to the difference of time occupied in the passage of the two portions, which thus proceed in parallel directions to the spectator's eye, after having been differently refracted and reflected within the drop. This difference varies at first, nearly as the square of the angular distance from the primitive rainbow: and, if the first additional red be at the distance of 2° from the red of the rainbow, so as to interfere a little with the primitive violet, the fourth additional red will be at a distance of nearly 2° more; and the intermediate colours will occupy a space nearly equal to the original rainbow. In order to produce this effect, the drops must be about $\frac{1}{76}$ of an inch, or .013, in diameter: it would be sufficient if they were between $\frac{1}{70}$ and $\frac{1}{80}$. The reason that such supernumerary colours are not often seen, must be, that it does not often happen that drops so nearly equal are found together: but, that this may sometimes happen, is not in itself at all improbable: we measure even medicines by dropping them from a phial, and it may easily be conceived that the drops formed by natural operations may sometimes be as uniform as any that can be produced by art. How accurately this theory coincides with the observation, may best be determined from Dr. LANGWITH's own words.

" August the 21st, 1722, about half an hour past five in the " evening, weather temperate, wind at north-east, the appearance " was as follows. The colours of the primary rainbow were as " usual, only the purple very much inclining to red, and well

"defined: under this was an arch of green, the upper part of "which inclined to a bright yellow, the lower to a more dusky "green: under this were alternately two arches of reddish "purple, and two of green: under all, a faint appearance of "another arch of purple, which vanished and returned several "times so quick, that we could not readily fix our eyes upon it. "Thus the order of the colours was, I. Red, orange-colour, yel-"low, green, light blue, deep blue, purple. II. Light green, dark "green, purple. III. Green, purple. IV. Green, faint vanishing "purple. You see we had here four orders of colours, and per-"haps the beginning of a fifth: for I make no question but that "what I call the purple, is a mixture of the purple of each of "the upper series with the red of the next below it, and the "green a mixture of the intermediate colours. I send you not "this account barely upon the credit of my own eyes; for there "was a clergyman and four other gentlemen in company, "whom I desired to view the colours attentively, who all "agreed, that they appeared in the manner that I have now de-"scribed. There are two things which well deserve to be taken "notice of, as they may perhaps direct us, in some measure, to "the solution of this curious phenomenon. The first is, that the "breadth of the first series so far exceeded that of any of the "rest, that, as near as I could judge, it was equal to them all "taken together. The second is, that I have never observed "these inner orders of colours in the lower parts of the rainbow, "though they have often been incomparably more vivid than "the upper parts, under which the colours have appeared. I "have taken notice of this so very often, that I can hardly look "upon it to be accidental; and, if it should prove true in general, "it will bring the disquisition into a narrow compass; for it will

"show that this effect depends upon some property which the "drops retain, whilst they are in the upper part of the air, but "lose as they come lower, and are more mixed with one ano- "ther." Phil. Trans. Vol. XXXII. p. 243.

From a consideration of the nature of the secondary rainbow, of 54°, it may be inferred, that if any such supernumerary colours were seen attending this rainbow, they would necessarily be external to it, instead of internal. The circles sometimes seen encompassing the observer's shadow in a mist, are perhaps more nearly related to the common colours of thin plates as seen by reflection.

IV. ARGUMENTATIVE INFERENCE RESPECTING THE NATURE OF LIGHT.

The experiment of GRIMALDI, on the crested fringes within the shadow, together with several others of his observations, equally important, has been left unnoticed by NEWTON. Those who are attached to the NEWTONIAN theory of light, or to the hypotheses of modern opticians, founded on views still less enlarged, would do well to endeavour to imagine any thing like an explanation of these experiments, derived from their own doctrines; and, if they fail in the attempt, to refrain at least from idle declamation against a system which is founded on the accuracy of its application to all these facts, and to a thousand others of a similar nature.

From the experiments and calculations which have been premised, we may be allowed to infer, that homogeneous light, at certain equal distances in the direction of its motion, is possessed of opposite qualities, capable of neutralising or destroying each

other, and of extinguishing the light, where they happen to be united; that these qualities succeed each other alternately in successive concentric superficies, at distances which are constant for the same light, passing through the same medium. From the agreement of the measures, and from the similarity of the phenomena, we may conclude, that these intervals are the same as are concerned in the production of the colours of thin plates; but these are shown, by the experiments of Newton, to be the smaller, the denser the medium; and, since it may be presumed that their number must necessarily remain unaltered in a given quantity of light, it follows of course, that light moves more slowly in a denser, than in a rarer medium: and this being granted, it must be allowed, that refraction is not the effect of an attractive force directed to a denser medium. The advocates for the projectile hypothesis of light, must consider which link in this chain of reasoning they may judge to be the most feeble; for, hitherto, I have advanced in this Paper no general hypothesis whatever. But, since we know that sound diverges in concentric superficies, and that musical sounds consist of opposite qualities, capable of neutralising each other, and succeeding at certain equal intervals, which are different according to the difference of the note, we are fully authorised to conclude, that there must be some strong resemblance between the nature of sound and that of light.

I have not, in the course of these investigations, found any reason to suppose the presence of such an inflecting medium in the neighbourhood of dense substances as I was formerly inclined to attribute to them; and, upon considering the phenomena of the aberration of the stars, I am disposed to believe, that the luminiferous ether pervades the substance of all material

bodies with little or no resistance, as freely perhaps as the wind passes through a grove of trees.

The observations on the effects of diffraction and interference, may perhaps sometimes be applied to a practical purpose, in making us cautious in our conclusions respecting the appearances of minute bodies viewed in a microscope. The shadow of a fibre, however opaque, placed in a pencil of light admitted through a small aperture, is always somewhat less dark in the middle of its breadth than in the parts on each side. A similar effect may also take place, in some degree, with respect to the image on the retina, and impress the sense with an idea of a transparency which has no real existence: and, if a small portion of light be really transmitted through the substance, this may again be destroyed by its interference with the diffracted light, and produce an appearance of partial opacity, instead of uniform semitransparency. Thus, a central dark spot, and a light spot surrounded by a darker circle, may respectively be produced in the images of a semitransparent and an opaque corpuscle; and impress us with an idea of a complication of structure which does not exist. In order to detect the fallacy, we may make two or three fibres cross each other, and view a number of globules contiguous to each other; or we may obtain a still more effectual remedy by changing the magnifying power; and then, if the appearance remain constant in kind and in degree, we may be assured that it truly represents the nature of the substance to be examined. It is natural to inquire whether or no the figures of the globules of blood, delineated by Mr. Hewson in the Phil. Trans. Vol. LXIII. for 1773, might not in some measure have been influenced by a deception of this kind: but, as far as I have hitherto been able to examine the globules, with a lens of one-fiftieth of an inch

focus, I have found them nearly such as Mr. HEWSON has described them.

V. REMARKS ON THE COLOURS OF NATURAL BODIES.

Exper. 5. I have already adduced, in illustration of NEWTON'S comparison of the colours of natural bodies with those of thin plates, Dr. WOLLASTON'S observations on the blue light of the lower part of a candle, which appears, when viewed through a prism, to be divided into five portions. I have lately observed a similar instance, still more strongly marked, in the light transmitted by the blue glass sold by the opticians. This light is separated by the prism into seven distinct portions, nearly equal in magnitude, but somewhat broader, and less accurately defined, towards the violet end of the spectrum. The first two are red, the third is yellowish green, the fourth green, the fifth blue, the sixth bluish violet, and the seventh violet. This division agrees very nearly with that of the light reflected by a plate of air $\frac{1}{6840}$ of an inch in thickness, corresponding to the 11th series of red and the 18th of violet. A similar plate of a metallic oxide, would perhaps be about $\frac{1}{15000}$ of an inch in thickness. But it must be confessed, that there are strong reasons for believing the colouring particles of natural bodies in general to be incomparably smaller than this; and it is probable that the analogy, suggested by NEWTON, is somewhat less close than he imagined. The light reflected by a plate of air, at any thickness nearly corresponding to the 11th red, appears to the eye to be very nearly white; but, under favourable circumstances, the 11th red and the neighbouring colours may still be distinguished. The light of some kinds of coloured glass is pure red; that of others, red with a little green: some intercept all the light, except the extreme

red and the blue. In the blue light of a candle, expanded by the prism, the portions of each colour appear to be narrower, and the intervening dark spaces wider, than in the analogous spectrum derived from the light reflected from a thin plate. The light of burning alcohol appears to be green and violet only. The pink dye sold in the shops, which is a preparation of the carthamus, affords a good specimen of a yellow green light regularly reflected, and a crimson probably produced by transmission.

VI. EXPERIMENT ON THE DARK RAYS OF RITTER.

Exper. 6. The existence of solar rays accompanying light, more refrangible than the violet rays, and cognisable by their chemical effects, was first ascertained by Mr. RITTER: but Dr. WOLLASTON made the same experiments a very short time afterwards, without having been informed of what had been done on the Continent. These rays appear to extend beyond the violet rays of the prismatic spectrum, through a space nearly equal to that which is occupied by the violet. In order to complete the comparison of their properties with those of visible light, I was desirous of examining the effect of their reflection from a thin plate of air, capable of producing the well known rings of colours. For this purpose, I formed an image of the rings, by means of the solar microscope, with the apparatus which I have described in the Journals of the Royal Institution, and I threw this image on paper dipped in a solution of nitrate of silver, placed at the distance of about nine inches from the microscope. In the course of an hour, portions of three dark rings were very distinctly visible, much smaller than the brightest rings of the coloured image, and coinciding very nearly, in their dimensions, with the

rings of violet light that appeared upon the interposition of violet glass. I thought the dark rings were a little smaller than the violet rings, but the difference was not sufficiently great to be accurately ascertained; it might be as much as $\frac{1}{30}$ or $\frac{1}{40}$ of the diameters, but not greater. It is the less surprising that the difference should be so small, as the dimensions of the coloured rings do not by any means vary at the violet end of the spectrum, so rapidly as at the red end. For performing this experiment with very great accuracy, a heliostate would be necessary, since the motion of the sun causes a slight change in the place of the image; and leather, impregnated with the muriate of silver, would indicate the effect with greater delicacy. The experiment, however, in its present state, is sufficient to complete the analogy of the invisible with the visible rays, and to show that they are equally liable to the general law which is the principal subject of this Paper. If we had thermometers sufficiently delicate, it is probable that we might acquire, by similar means, information still more interesting, with respect to the rays of invisible heat discovered by Dr. Herschel; but at present there is great reason to doubt of the practicability of such an experiment.

II. *Continuation of an Account of a peculiar Arrangement in the Arteries distributed on the Muscles of slow-moving Animals, &c. In a Letter from Mr.* Anthony Carlisle *to* John Symmons, *Esq. F. R. S.*

Read December 8, 1803.

DEAR SIR,

You did me the honour of presenting to the Royal Society, An Account of a Peculiarity in the Distribution of the Arteries sent to the Limbs of slow-moving Animals.* According to my intention expressed in that letter, I have, since that time, endeavoured to collect farther illustrations of the connection between the disposition of the blood-vessels and the actions of muscles. Neither the tribe of ruminating, nor the carnivorous animals, have afforded the evidence which I had expected, from the investigation of their masseter, pterygoid, and temporal muscles; since these are all supplied by the ordinary arborescent arteries. The rete mirabile, in such animals, seems to be a contrivance to restrain that velocity of the blood which their habits and figures would otherwise produce, in its passage to the brain. The circuitous course of all the arteries which supply the human brain, and their confinement in bony passages, is obviously for a similar purpose. Having sought for examples in other directions, the following results have occurred.

In the human body, two small arteries arise from the upper

* See Phil. Trans. for 1800, p. 98.

part of the abdominal aorta, and, in men, descend from thence to the testicles: they are usually but two in number, and are called the spermatic arteries. Through an extent of ten inches, by average computation, these vessels give off comparatively few lateral branches, so that they may be properly considered as the longest arterial cylinders in the body. The spermatic arteries supply the muscles called cremasters, which suspend the testicles, and the fibrils of the dartos, which corrugates the scrotum. The slow actions of these muscles, and their occasional long continuance in a contracted state, are sufficiently known. The next in resemblance are the intercostal arteries; and lastly those of the diaphragm. Now the slowness of the muscular actions in respiration, and the occasional duration of arbitrary actions in these muscles, need only to be mentioned. Whether the peristaltic motions of the alimentary canal be influenced by the circuitous course of some of the arteries, and their numerous junctions, I am unable to determine; but these arteries are distributed differently, in those respects, from the arteries of ordinary muscles.

The iris, in man, and in animals, is furnished with cylindrical arteries, which pierce the posterior part of the globe of the eye, and finally enter that muscle by a circuitous course.* The pupil of the eye contracts slowly, and is occasionally required to continue long in that state.

The instance of an opposite mode of distribution, is to be found in the coronary arteries which supply the heart, a muscle whose actions are more rapid than those of any other; at the same time it is observable, that the coronary arteries are more

* Vide ZINN. *Descriptio anatomica Oculi humani,* Plate 3, Fig. 2, *bb,* and Fig. 1, *mn.*

quickly subdivided than the arteries of muscles generally. The alteration which the blood undergoes during the supply of this muscle only, is worthy of remark, *viz.* that the coronary veins return venous blood apparently as much changed from the arterial state, as if it had passed through the remotest organs of the body.

Any impediment to the accustomed course of the blood flowing through muscles, induces a corresponding diminution in their powers of action. When the principal arterial trunk which supplies the muscles of the leg is obliterated by ligature, for the cure of an aneurism, the leg remains afterwards much weakened in its muscular strength, until the circuitous vessels have again restored a vigorous supply of blood.

Animals with prehensile tails, such as certain monkeys and opossums, have the muscles of their tails supplied by one cylindrical artery; and the length of time they can suspend themselves by their tails is remarkable, notwithstanding the assistance derived from the repetition of coils around the bough of a tree, and occasionally from an elastic bend in the extreme joints of some prehensile tails.

The swimming-bladders of some fishes afford another example of cylindrical arteries supplying the muscular parts of them. (See Plate I. Fig. 1.) That these parts are truly muscular, I have ascertained by their excitation with the GALVANIC metals. The swimming-bladders appear destined to assist the fish in rising or descending in the water, as well as to keep the back upwards when at rest; so that their muscular actions are probably of slow performance, and require to be of long continuance.

The intestinum ileum of the Cavia Aguti has a similar disposition of blood-vessels; from which I was led to consider the

arteries of the alimentary canal in other animals; and, in many of them, this extension of undivided vessels is to be found. See Fig. 2.

Many of the amphibious class of animals are slow in their motions, such as the tortoises, lizards, and toads; but, whether they can also continue their muscular actions longer than in ordinary cases, I do not know. The blood circulates more slowly in the amphibia; and their respiration is not so important to the vital functions. It cannot however be omitted, that many of the serpents, and some lizards, are very agile.

During the state of contraction, as has been often noticed, the muscles of animals with red blood become of a paler colour, and recover their former redness on the subsequent relaxation; it may therefore be affirmed, that muscular fibres are not distended with blood when in the state of contraction, but that the replenishing with blood is to supply some fresh material, which is employed in muscular action. Whatever substance this may be, it is less required for the irritability of the muscles in some animals than in others. Temperature, and the organs of respiration, seem to be intimately connected with these differences; but all illustrations of such points would extend beyond the limits of the present inquiry.

It has been shewn, that slowness of muscular action, and extraordinary duration of the contractions, are frequently united; and that such unusual phenomena in muscles, are accompanied with a peculiar distribution of the arteries which supply them: but, whether the slowness or the duration be the principal end, or whether the equable supply of blood by a set of appropriate arteries, be the only adaptation convenient for the peculiar offices of such muscles, are subjects not easily determined.

I have considered the accompanying figures of the arteries on the swimming-bladder of the tench, as a striking additional illustration of the subject, though they have been often noticed before, and occasionally ill represented. The intestinum ileum of the aguti is also figured, to show the same kind of distribution. The vessels in both having been injected, for the purpose of a clearer representation.

References to the Figures, Plate I.

Fig. 1 represents a side view of the double swimming-bladder of the tench. *(Cyprinus Tinca.)* The arteries on one side of the hinder bladder being drawn as they appear when injected. The upper portion of the bladder is devoid of those kind of vessels, and does not appear to be muscular. In the hinder portion of the swimming-bladder of the barbel, *(Cyprinus Barbus,)* these parallel cylinders wind spirally round the bladder, instead of proceeding straight forward along the sides.

a, The ductus pneumaticus entering the hinder portion of the vesica aerea.

b, The trunk of the artery from whence the bundles of cylinders are distributed.

c, The upper portion of the vesica aerea, which is covered by a thick, white, opaque, tender membrane.

Fig. 2. The intestinum ileum, and part of the cæcum, of the Cavia Aguti; the vessels being distended with quicksilver.

a, a, a, The intestinum ileum.

b, Part of the appendix vermiformis.

c, Part of the cæcum.

d, d, Anastomosing vessels, crossing the cylinders at right angles, and uniting with each of them.

Future researches in natural history seem to promise a more ample, and a clearer view, of the gross arrangements which attend the structure of muscles; a state of knowledge which will lead physiologists to determine between the essential connections, and the appendages of convenience belonging to muscles, and thereby to see more correctly, the immediate phenomena of muscular actions.

I am, &c.

ANT. CARLISLE.

Soho Square,
Nov. 15, 1803.

Fig. 1.

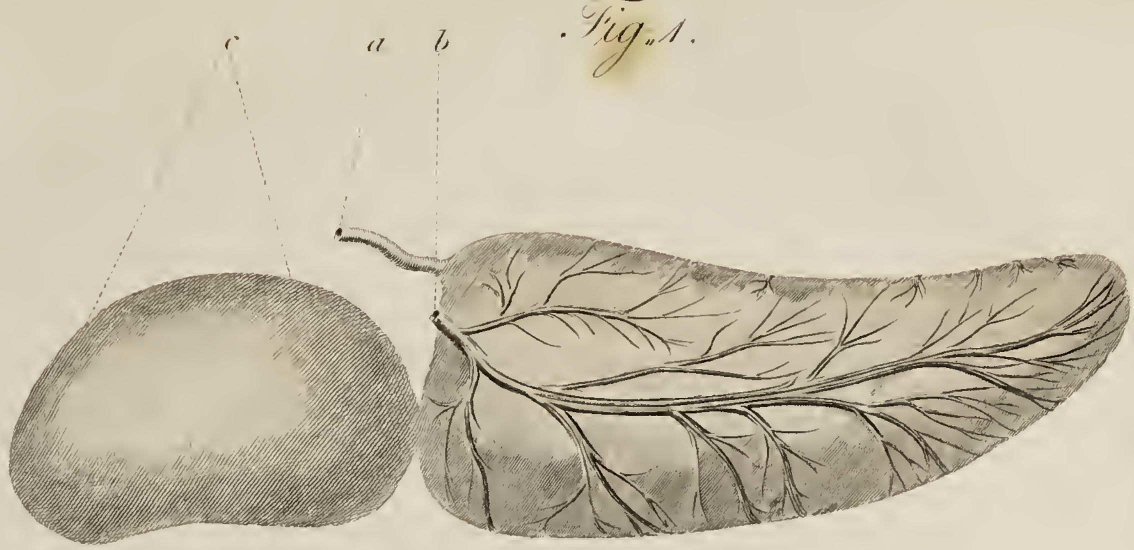
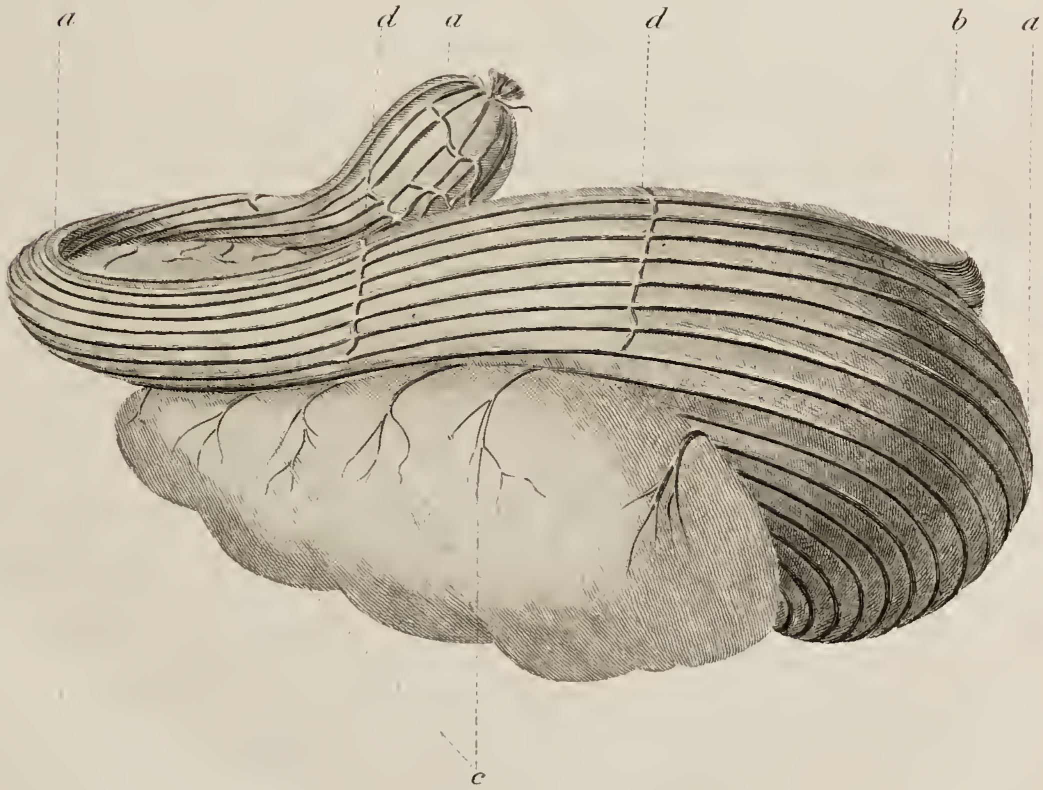


Fig. 2.



Basire sc.

III. *An Account of a curious Phenomenon observed on the Glaciers of Chamouny; together with some occasional Observations concerning the Propagation of Heat in Fluids. By* Benjamin *Count of* Rumford, *V. P. R. S. Foreign Associate of the National Institute of France, &c. &c.*

Read December 15, 1803.

In an excursion which I made the last summer, in the month of August, to the Glaciers of Chamouny, in company with Professor Pictet of Geneva, I had an opportunity of observing, on what is called the Sea of Ice, (*Mer de Glace,*) a phenomenon very common, as I was told, in those high and cold regions, but which was perfectly new to me, and engaged all my attention. At the surface of a solid mass of ice, of vast thickness and extent, we discovered a pit, perfectly cylindrical, about seven inches in diameter, and more than four feet deep; quite full of water. On examining it on the inside, with a pole, I found that its sides were polished; and that its bottom was hemispherical, and well defined.

This pit was not quite perpendicular to the plane of the horizon, but inclined a little towards the south, as it descended; and, in consequence of this inclination, its mouth or opening, at the surface of the ice, was not circular, but elliptical.

From our guides I learnt, that these cylindrical holes are frequently found on the level parts of the ice; that they are formed during the summer, increasing gradually in depth, as long

as the hot weather continues; but that they are frozen up, and disappear, on the return of winter.

I would ask those who maintain that water is a conductor of heat, how these pits are formed? On a supposition that there is no direct communication of heat between neighbouring particles of that fluid, which happen to be at different degrees of temperature, the phenomenon may easily be explained; but it appears to me to be inexplicable on any other supposition.

The quiescent mass of water, by which the pit remains constantly filled, must necessarily be at the temperature of freezing; for it is surrounded on every side by ice: but the pit goes on to increase in depth, during the whole summer. From whence comes the heat that melts the ice continually at the bottom of the pit? and how does it happen, that this heat acts on the *bottom* of the pit only, and not on its sides?

These curious phenomena may, I think, be explained in the following manner. The warm winds which, in summer, blow over the surface of this column of ice-cold water, must undoubtedly communicate some small degree of heat to those particles of the fluid with which this warm air comes into immediate contact; and the particles of the water at the surface so heated, being rendered specifically heavier than they were before, by this small increase of temperature, sink slowly to the bottom of the pit; where they come into contact with the ice, and communicate to it the heat by which the depth of the pit is continually increased.

This operation is exactly similar to that which took place in one of my experiments, (See my Essay on the Propagation of heat in Fluids, *Experiment* 17,) the results of which, no person, to my knowledge, has yet explained.

There is another very curious natural phenomenon, which I could wish to see explained in a satisfactory manner, by those who still refuse their assent to the opinions I have been led to adopt, respecting the manner in which heat is propagated in fluids. The water at the bottoms of all deep lakes is constantly at the same temperature, (that of 41° FAHRENHEIT,) summer and winter, without any sensible variation. This fact alone appears to me to be quite sufficient to prove, that if there be any immediate communication of heat between neighbouring particles or molecules of water, *de proche en proche*, or from one of them to the other, that communication must be so extremely slow, that we may with safety consider it as having no existence; and it is with this limitation that I beg to be understood, when I speak of fluids as being non-conductors of heat.

In treating of the propagation of heat in fluids, I have hitherto confined myself to the investigation of the simple matter of fact, without venturing to offer any conjectures relative to the causes of the phenomena observed. But the results of recent experiments on the calorific and frigorific radiations of hot and of cold bodies, (an account of which I shall have the honour of laying before the Royal Society in a short time,) have given me some new light respecting the nature of heat, and the mode of its communication; and I have hopes of being able to show *why* all changes of temperature, in *transparent* liquids, must necessarily take place at their surfaces.

I have seen with real pleasure, that several ingenious gentlemen, in London, and in Edinburgh, have undertaken the investigation of the phenomena of the propagation of heat in fluids; and that they have made a number of new and ingenious experiments, with a view to the farther elucidation of that

most interesting subject. If I have hitherto abstained from taking public notice of their observations on the opinion I have advanced on that subject, in my different publications, it was not from any want of respect for those gentlemen that I remained silent, but because I still found it to be quite impossible to explain the results of my own experiments, on any other principles than those which, on the most mature and dispassionate deliberation, I had been induced to adopt; and because my own experiments appeared to me to be quite as conclusive (to say no more of them) as those which were opposed to them; and, lastly, because I considered the principal point in dispute, relative to the passage of heat in fluids, as being so clearly established by the circumstances attending several great operations of nature, that this evidence did not appear to me to be in danger of being invalidated by conclusions drawn from partial and imperfect experiments, and particularly from such as are allowed on all hands to be extremely delicate.

In all our attempts to cause heat to descend in liquids, the heat unavoidably communicated to the sides of the containing vessel, must occasion great uncertainty with respect to the results of the experiment; and, when that vessel is constructed of ice, the flowing down of the water resulting from the thawing of that ice, will cause motions in the liquid, and consequently inaccuracies of still greater moment, as I have found from my own experience; and, when thermometers immersed in a liquid, at a small distance below its surface, acquire heat, in consequence of a hot body being applied to the surface of the liquid, that event is no decisive proof that the heat acquired by the thermometer is communicated by the fluid, from above, downwards, from molecule to molecule, *de proche en proche;* so far from

being so, it is not even a proof that it is from the fluid that the thermometer receives the heat which it acquires; for it is possible, for aught we know to the contrary, that it may be occasioned by the radiation of the hot body placed at the surface of the fluid.

In the experiments of which I have given an account, in my Essay on the Propagation of Heat in Fluids, great masses, many pounds in weight, of boiling hot water, were made to repose for a long time (three hours) on a cake of ice, without melting but a very small portion of it; and, on repeating the experiment with an equal quantity of very cold water, (namely, at the temperature of 41° FAHRENHEIT,) nearly twice as much ice was melted in the same time. In these experiments, the causes of uncertainty above mentioned did not exist: and the results of them were certainly most striking.

The conclusions which naturally flow from those results, have always appeared to me to be so perfectly evident and indisputable, as to stand in no need, either of elucidation, or of farther proof.

If water be a conductor of heat, how did it happen that the heat in the boiling water did not, in three hours, find its way downwards to the cake of ice, on which it reposed, and from which it was separated only by a stratum of cold water, half an inch in thickness?

I wish that gentlemen who refuse their assent to the opinions I have advanced respecting the causes of this curious phenomenon, would give a better explanation of it than that which I have ventured to offer. I could likewise wish that they would inform us how it happens, that the water at the bottoms of all deep lakes remains constantly at the same temperature: and, above all, how the cylindrical pits, above described, are formed

in the immense masses of solid and compact ice which compose the Glaciers of Chamouny?

A remark, which surprised me not a little, has been made by a gentleman of Edinburgh, (Dr. Thomson,) on the experiments I contrived, to render visible the currents into which liquids are thrown on a sudden application of heat, or of cold. He conceives, that the motions observed in my experiments, among the small pieces of amber which were suspended in a weak solution of potash in water, were no proof of currents existing in that liquid; as they might, in his opinion, have been occasioned by a change of specific gravity in the amber, or by air attached to it. I am sorry that so mean an opinion of my accuracy as an observer should have been entertained, as to imagine that I could have been so easily deceived. For nothing surely is easier, than to distinguish the motion of a solid suspended in a liquid of the same specific gravity, which is carried along by a current in the liquid, from that of a body which descends, or ascends, in the liquid, in consequence of its relative weight, or levity. In the one case, the motion is uniform; in the other, it is accelerated. In a current, the body may be carried forward in all directions, and even in curved lines; but, when it falls in a quiescent fluid, by the action of gravity, or rises, in consequence of its being specifically lighter than the fluid, it must necessarily move in a vertical direction.

The fact is, that I very often observed, in the course of my numerous experiments, the motions of small particles of matter, of different kinds, in water, which Dr. Thomson describes; but, so far from inferring *from them* the existence of currents in that fluid, their cause was so perfectly evident, that I did not even think it necessary to make any mention of them.

I cannot conclude this Paper, without requesting that the Royal Society would excuse the liberty I have taken in troubling them with these remarks. Very desirous of avoiding every species of altercation, I have hitherto cautiously abstained from engaging in literary disputes; and I shall most certainly endeavour to avoid them in future.

I am responsible to the public for the accuracy of the accounts which I have published of my experiments; but it cannot reasonably be expected, that I should answer all the objections that may be made to the conclusions which I have drawn from them. It will however, at all times, afford me real satisfaction to see my opinions examined, and my mistakes corrected; for my first and most earnest wish is, to contribute to the advancement of useful knowledge.

IV. *Description of a triple Sulphuret, of Lead, Antimony, and Copper, from Cornwall; with some Observations upon the various Modes of Attraction which influence the Formation of mineral Substances, and upon the different Kinds of Sulphuret of Copper. By the Count de* Bournon, *F. R. S. and L. S.*

Read December 22, 1803.

SULPHURET OF LEAD, ANTIMONY, AND COPPER.

This substance, which has hitherto been found only in Cornwall, has been many years known in England. Most of the collections of minerals in London contain specimens of it, in which its peculiar characters are more or less distinctly seen; nevertheless, either the scarcity of this mineral (the mine* in in which it is found having never been regularly worked) has been an obstacle to its being more known in Europe, or the nature of its external characters, which do not allow it to be classed with any of the mineral substances already described, has prevented mineralogists from giving it a place in their works; for, it is very certain that none of them, not even those who have lately written, have taken any notice of it.

In the country where this mineral is produced, it has been generally considered as an antimonial ore; but no observation, at least none that has ever come to our knowledge, had yet determined, whether it was an ore of antimony, properly so called,

* Huel Boys, in the northern part of Cornwall.

or one of those compound metallic bodies in which various metals are combined with the same mineralizing substance, as is frequently observed in mineralogy. At length Mr. Hatchett, to whom the science of mineralogy is already under many obligations, has added to them, that of having determined, in the most satisfactory manner, the place this substance ought to occupy amongst metallic bodies. Mr. Hatchett has ascertained, that it is a triple sulphuret, in which the sulphur is combined, at the same time, with lead, antimony, and copper. Mineralogy has hitherto furnished very few instances of triple metallic sulphurets, particularly of such as (like that here described) exhibit their characters in as striking a manner to the mineralogist as to the chemist.

The colour of this mineral is a dark gray, inclining to black.

It has a very brilliant lustre.

It is very brittle; fragments of it may be easily broken off by means of the nail.

Its hardness is such, that it very easily cuts calcareous spar; but it is not sufficiently hard to scratch fluor spar.

When rubbed pretty strongly on white paper, it leaves on it a faint black mark; but not so readily as lead, or sulphuret of antimony.

It does not, when rubbed, emit any smell.

When grossly powdered, the powder still retains the metallic lustre.

When thrown, in the last mentioned state, upon an iron not quite red hot, it emits a phosphorescent light, of a bluish-white colour, but without any smell whatever; no such light, however, can be obtained from it by means of friction.

Its specific gravity is 5765;* it is consequently superior to that of sulphuret of copper, or sulphuret of antimony, but very inferior to that of sulphuret of lead.

The fractures of its crystals are not smooth, neither are they lamellated in any particular direction, but are generally granulated, and have rather a coarse grain.

The crystals of this substance are very brilliant, and often of a very large size. I have seen some that were more than an inch in length, and of a proportional height: but, as most of them have a great number of facets, and are frequently very irregularly shaped, on account of the inequality of their increase, it becomes very difficult to determine their form; particularly as the crystals most commonly found, are those which differ most from the primitive form, to which, on that account, it becomes very difficult to refer them. For this reason, I have resolved to give a particular description of every thing that relates to the crystalline forms of the substance; hoping that I shall, by that means, contribute to promote the knowledge of a character so important in the study of mineralogy.

Primitive Crystal. The form of the primitive crystal is a rectangular tetraedral prism, which has its terminal faces perpendicular to its axis, as in Plate II. Fig. 1; but I have never yet observed, in all the specimens I have seen of this substance, the above-mentioned prism entirely destitute of secondary facets.

* Of two trials of the specific gravity of this substance, taken at six months distance from each other, the first gave me 5765, the second 5763. It is evident that it is scarcely possible to approach more nearly to that found by Mr. HATCHETT, namely, 5766; and this constancy in the above character, may be considered as a proof of the state of purity habitual to this sulphuret.

*First Modification.** This modification consists in a decrease along the vertical edges of the primitive crystal, so that each of these edges is replaced by a plane, which is equally inclined upon the adjacent faces, and which consequently makes, with each of them, an angle of 135°, as in Fig. 2. Among the crystals I have hitherto seen of this substance, I have never yet met with this simple octaedral prism; it is usually combined with some of the other modifications, and, in that case, the sides of the secondary prism are generally broader than those of the primitive crystal, as is represented in Fig. 3. The new planes produced by this first modification, are usually striated, and often very deeply, in their vertical direction.†

When the new planes abovementioned have acquired sufficient extent to cause the vertical faces of the primitive crystal entirely to disappear, the result is, a rectangular tetraedral prism, perfectly similar to that of the primitive crystal, excepting that, in all I have met with, I have constantly observed that the vertical faces were striated, as in Fig. 4. This kind of prism is rare in this substance; I have, however, met with two or three instances

* I commonly make use of the term *modification*, to express that alteration which takes place in the primitive crystal, whenever the crystalline laminæ (which are themselves only a regular collection of molecules) undergo a change with respect to the ratio of the arithmetical progression they admit, in their deviation from the edges or angles of the primitive crystal; and I employ the word *variety*, to signify those differences in the crystals which arise only from the various proportions of the faces to each other, or those which are produced by the combination of two or more modifications in the same crystal.

† Although, in all these crystals, the above-mentioned striæ are owing to an imperfection in their crystallization, I have, in the figures, represented by strokes, those striæ which distinguish the planes of the secondary prism of this substance. By this means, it becomes more easy to comprehend the manner in which the crystals differ from each other.

of it. The above-mentioned vertical striæ are sometimes so strongly marked, as to give reason to suppose we might, by a very trifling effort, succeed in separating the laminæ from each other, according to their natural direction; but every attempt I have made with this intention, has constantly been fruitless.

Second Modification. This modification is produced by a decrease at the solid angles of the primitive crystal, in consequence of which, each of these angles is replaced by a plane, which makes, at its meeting with the terminal faces, an angle of 130°; and, with the vertical edges of the prism, an angle of 140°, as in Fig. 5. I have never yet observed the planes produced by this modification, simply combined (as is shown in the figure) with those of the primitive crystal; but the crystals I shall now describe, will demonstrate the probability of the existence of such a combination. I have, however, seen these planes combined with those of the secondary prism; and, when that happens, the planes caused by this second modification, instead of being situated at the solid angles of the prism, (as is the case when they are combined with the planes of the primitive crystal,) are situated on the edges of the terminal faces.

Third Modification. This modification is also produced by a decrease at the solid angles of the primitive crystal; but this decrease is greater than the former, that is to say, the edges of the laminæ, which are deposited one upon the other, recede in a greater degree from the edges of those which have preceded them. This gives rise to a plane, which, in a similar manner, replaces the solid angles of the primitive crystal, and makes, at its meeting with the terminal faces, an angle of 150°; and, with the edges of the prism, one of 120°. See Fig. 6. I have not indeed hitherto seen the planes produced by this third modification

combined merely with those of the primitive crystal; but it is yet more common than in the preceding modification, to meet with them combined with the planes of the secondary prism; and they are then situated along the edges of the terminal faces, as is shown in Fig. 7. More frequently, however, the prism is very short, as is represented in Fig. 8; and it usually preserves some traces, more or less considerable, of the sides of the primitive crystal, as in Fig. 9.

It sometimes happens, that the decrease producing this modification has taken place only in two of the opposite angles, situated in the same direction, on the terminal faces; then, if the planes arising from this modification happen to be combined with those of the secondary prism, they are situated only on two opposite edges of each of the terminal faces of this prism. See Fig. 10. I have observed this variety in pretty large crystals; and the specimen in which I met with them (and which, like all the others, came from Cornwall) contains also some very regular aggregations, each consisting of four of these crystals, in the form of a cross. These crystals penetrate each other for a certain part of their extent, as is shown in Fig. 11.

If the planes produced by this third modification were combined with those arising from the second modification, the result would be, the variety represented in Fig. 12, in which the planes of the two last modifications meet together at an angle of 170°; but I have not yet met with this variety in so simple a state.

Fourth Modification. This is produced by a decrease along the edges of the terminal faces of the primitive crystal; in consequence of which, each of those edges is replaced by a plane, which is equally inclined upon the adjacent faces, and makes,

with the terminal faces, and with the vertical faces of the prism, an angle of 135°, as in Fig. 13.

If the planes produced by this fourth modification were combined with those of the secondary prism, they would then be situated at the solid angles of this prism, as is represented in Fig. 14. I have not yet seen these two last varieties in so simple a state as that in which the figures represent them; but the two most common forms in which this substance has hitherto been found, are those shewn in Figs. 15 and 16. Now, Fig. 15 is nothing more than the combination of the planes of the primitive crystal with those of this modification, and with those of the first and second modifications. In the same way, Fig. 16 is likewise the result of the same combination; but with the planes of the third modification added to it.

Lastly, Fig. 17 exhibits a detached crystal, of a perfectly determinate form, and of about an inch in length, seven lines in breadth, and five lines in height, which makes part of the collection of Mr. Phillips, who was so kind as to allow me to examine it, as well as every other specimen of this substance in his possession. It is, in fact, the variety represented in Fig. 16, but in which the planes belonging to the fourth modification have acquired a more considerable extent; while those which belong to the third modification exist only on two of the opposite edges of the terminal faces, as was observed in speaking of the crystal represented in Fig. 10.

Before I proceed to those observations which this triple sulphuret (a most interesting substance in mineralogy) affords me an opportunity of making, with regard to the various ores that are produced by the combination of sulphur and copper, of the nature of which, neither mineralogy nor chemistry have yet

supplied any certain information, I think it right to make a few remarks, upon some circumstances relating to the different modes of attraction that appear to influence the formation of mineral substances. However imperfect this sketch may be, it will at least serve to illustrate what I shall hereafter say, respecting the various ores I have just mentioned.

OBSERVATIONS ON THE VARIOUS MODES OF ATTRACTION WHICH INFLUENCE THE FORMATION OF MINERAL SUBSTANCES.

The observations to which the study of mineralogy, which of late years has been pursued with particular attention, has given rise, seem to me to lead to the following conclusion, *viz.* that, of the two kinds of attraction which have been hitherto admitted to prevail in the formation of mineral substances, namely, the *attraction of composition*, and the *attraction of aggregation*, the latter is subject to different modes of action, all of which have a striking effect in the formation of mineral substances.

The first kind of attraction to which mineral bodies are subject, and which is generally known by the name of chemical attraction, is the *attraction of composition.* This kind of attraction takes place only between the most simple or primitive molecules of a substance; but, at the same time, it exists only between molecules that are dissimilar, or that belong to different substances. To its action is owing the formation of new molecules, to which may be properly given the name of *secondary* or *integrant* molecules; because they, and they only, determine the nature of all the compound bodies belonging to the mineral kingdom. These molecules are the result of the intimate com-

bination, in different proportions, of the primitive molecules of two or more different substances. The difference existing between mineral bodies, consequently depends upon the following circumstances; first, upon the nature of the primitive molecules, by the combination of which they are produced; secondly, upon the proportion in which those molecules are combined together.

At the instant the new or secondary molecules are formed, if they happen to be at a proper distance from each other, and in a fluid which permits them to move freely, they become subject to, and are forced to obey, the second species of attraction, namely, the attraction of aggregation, which unites them into one or several masses, perfectly homogeneous in all their parts.

But the attraction of aggregation seems to be susceptible of various modifications, which alter its manner of acting upon the constituent molecules. Of these modifications there are two principal ones; the first of which may, I think, be distinguished by the name of *crystalline attraction of aggregation*; the second may be called *simple attraction of aggregation.*

The crystalline attraction of aggregation always takes place between similar molecules; which molecules are simple or primitive, in those bodies which are considered as simple or primitive, (and which in fact are so,) and they are compound in other bodies. This kind of attraction, in its action upon the molecules under its influence, is either *regular*, *irregular*, or *amorphous.*

In the regular crystalline attraction of aggregation, the molecules arrange themselves in such a way as to give rise to solid bodies, which are either constantly of the same form, or are subject to certain laws of variation; these variations, however,

are always capable of being referred to the same primitive form. This kind of attraction, to exert the full action of which it is capable, requires (as well as the attraction of composition) that the molecules on which it acts should be situated in a fluid, in order that those molecules may possess a perfect freedom of motion. It is also necessary that this fluid be at rest; any motion foreign to that of the molecules themselves, disturbs them, and necessarily proves an obstacle to that regularity of form which is the natural result of their exact union. The fluid must, at the same time, be in a situation where it can evaporate gently; which evaporation, causing the molecules slowly to approach each other, brings them successively within their sphere of attraction. A crystalline mass may however show no regular determinate form, and nevertheless be the result of a regular crystallization. Such, for instance, is the formation of the pure transparent massive carbonate of lime, known by the name of calcareous Alabaster, which exactly fills the fissures or cavities in which it is found; such also is the formation of those stalactites which are composed of this kind of alabaster. In this case, as the cavities are completely filled, there cannot be formed in them any distinct crystal; and the above-mentioned masses may be considered as a large aggregation of crystals, the sides of which, being similar in form, and situated in the same direction, adhere together in one indeterminate mass. But, when that happens, the fracture of the mass always shows, in a satisfactory manner, the nature of its formation. In the substances I have just spoken of, for instance, the fractures are always lamellated; and, by following the direction of the lamellæ, we may always bring any fragment of the mass into the form of its primitive crystal.

When, in the formation of a substance that is acted upon by the crystalline attraction, its action happens to be disturbed, by any cause whatever, the masses resulting from it do not belong to the *regular* crystalline attraction; and the *irregular* crystalline attraction which takes place, produces various results, which no doubt are owing to many different causes. Sometimes there are masses partially lamellated, the lamellæ of which cross each other in various directions; at other times, we find masses which are either foliated, or fibrous.

It sometimes happens, (owing perhaps to a more considerable degree of disturbance during the process of attraction,) that there are formed small irregular detached masses, often so minute as to be scarcely perceptible; at other times, they are of a larger size, and, as soon as formed, fall to the bottom of the liquor, and unite together by a simple mode of attraction, which may with great propriety be called *simple homogeneous attraction of aggregation.* Of this kind are, granulated quartz, granulated carbonate of lime, &c. the different kinds of which substances, differ from each other only by the fineness or coarseness of their grain. Sometimes, however, the crystalline attraction of aggregation, and the simple homogeneous attraction, act together, at the same time, in the same solution. When this takes place, the granulated masses, instead of being composed of an aggregate of irregular grains, appear to consist of small crystals, which have a very regular form. This frequently takes place in pure granulated carbonate of lime, and still more frequently in magnesian carbonate of lime, particularly in that kind which is found in large and extensive masses, in various parts of England. In the case here spoken of, which, it may be presumed, can take place only in highly saturated solutions, in which the molecules

are consequently very near to each other, there is a rapid and detached crystallization, in all parts of the solution, while no attraction takes place between the crystals that fall down, except that of simple aggregation. I repeat, that no other than simple attraction of aggregation takes place between the crystals; for, if the crystalline attraction, which first took place, had continued to exert its action, the crystals, instead of joining together in a confused and irregular manner, would unite by their analogous sides, and produce one or several very large crystals; which crystals would be either exactly similar to the primitive crystals, (of which we see such frequent instances in fluate of lime, and in sulphuret of lead,) or they would have secondary planes, produced by a regular decrease, similar to those which have been mentioned and described by others, as being formed only by the molecules of the primitive crystalline form. These decreases can, in this case, only modify the secondary form; in the same way as the decreases produced by the primitive molecules, modify the primitive form. This new law, of which till now no notice has been taken, will perhaps appear at first view to militate against the generally received ideas of crystallization; its existence, however, cannot be doubted. In fluate of lime, and also in carbonate of lime, are frequently seen irrefragable proofs of it; indeed it appears to me that, far from contradicting the laws of attraction hitherto observed in nature, it agrees perfectly well with them.

Sometimes, in the great dissolutions of nature, the molecules, instead of uniting together by the influence of the crystalline attraction of aggregation, are precipitated in a detached but confused manner. In that case, the action of this mode of attraction has entirely ceased, and has given place to that of simple

aggregation; and the masses that result from it, no longer offer any appearance that can recal to the mind the known circumstances of crystallization. The fractures of the mass have no lamellated or regular aspect; and very often present that which is distinguished by the name of *earthy*. Such is, in various substances, the formation of most of those varieties to which Mr. WERNER has given the name of *compact*.

Many circumstances seem to lead to the idea that, in the formation of certain substances, there exists a species of crystallization by which no determinate form is produced. These substances, however, are really the result of a regular crystallization; that is to say, of as regular a crystallization as can take place in them, and one which cannot be referred to any of the disturbed crystallizations of which I have already spoken. This property depends, probably, upon the peculiar form of the primitive molecules of those substances; such, for instance, is the globular form, or those forms which approach to it: perhaps, however, it depends upon other causes. This species of attraction, is what I have called *amorphous attraction of crystallization;* a name that, in my opinion, expresses very properly the nature of its action, which appears not to allow the substances on which it acts, to take any determinate geometric form, however pure those substances may be, or under whatever circumstances that action may take place. Calcedony, girasol,* (which substance, when in a certain state of decay, is called opal,) and that kind of steatite which is perfectly pure and transparent, appear to me to owe their origin to this mode of formation.

* I have long since adopted this word, (which had already been given to one of the purest varieties of the above substance,) to distinguish the substance to which Mr. WERNER gives the general name of opal, and to which the Abbé HAUY gives the name of *quartz resinite*.

This subject requires that I should enter into it more minutely. In so doing, I shall make calcedony the basis of my observations, which may be afterwards easily applied to such other substances as are similarly circumstanced. I shall first observe, that calcedony, by the purity and homogeneity it is frequently seen to possess, as well as by its nature and the grain of its fracture, positively announces a state of crystallization; yet, although we frequently observe it in circumstances exactly similar to those which, in other substances, give rise to determinate crystals, (such as actual solution in a fluid, and that fluid inclosed in a cavity where it is in a state of rest,) it always offers itself to us with the same appearance, in which there cannot be perceived any tendency to a regular form. Every mineralogist is acquainted with the beautiful geodes of Oberstein, in the cavities of which are seen crystals of quartz, also of carbonate of lime, and very frequently, likewise, fine crystals of the substance called by Werner *Kreutzstein*, and by the Abbé Hauy *Harmotome*. Calcedony is frequently found with the above substances; but, when that happens, although it is very pure, and is in the same circumstances as those substances, it always appears either in the form of layers, or of mamillæ, or in the ramose form of stalactites; and never shows the least tendency to a determinate crystalline form. The same remark may be applied to other geodes, composed of agate, of flint, &c. Indeed, if calcedony were capable of assuming a determinate crystalline form, how does it happen that this substance, (which is one of the most common in nature, and is found in so many different forms, and so variously circumstanced,) has never yet been met with in a state that shewed any appearance of that form. It is true, that many mineralogists have spoken of crystals of calce-

dony; but, from the variety of forms they have assigned to these crystals, (which forms have no connection with each other, and evidently belong to other known substances,) it is very clear that all these pretended crystals of calcedony, have been produced merely by its having taken the place of other substances; either by being moulded in the cavities left by the destruction of the crystals of such substances, or by being formed at the very instant of the destruction of those crystals, by an operation analogous to that known by the name of cementation. Examples of the nature here spoken of, are well known to happen in many other substances. Besides, those stones which offer the greatest resistance to a mechanical division, such as quartz, blue corundum or sapphire, zircon, garnet, &c. sometimes show, by fortunate accidental fractures, or other natural accidents, the direction of the crystalline laminæ of which they are composed. Quartz, for instance, is sometimes found with an evidently laminated appearance, particularly the blue variety, which is brought to us either from Canada or from the East Indies; but never yet was an appearance of this nature perceived in any kind of calcedony.

Many celebrated mineralogists, amongst whom may be mentioned the Abbé Hauy, seem to explain this want of crystallization in calcedony, by considering that substance as nothing more than quartz in a concrete state. This is, in fact, supposing it a sort of irregular crystallization, which bears the same analogy to quartz, that calcareous alabaster bears to crystallized carbonate of lime. I cannot, however, by any means adopt this opinion, as it does not appear to me to agree with those facts which nature permits us to observe; for, not to mention those kinds of quartz which are produced by a disturbed crystallization,

for instance, granulated quartz, both of a fine and of a coarse grain, quartz in stalactites, &c. we are acquainted with several kinds of true concrete quartz, which forms veins in primitive rocks, in the same manner as carbonate of lime forms veins in secondary rocks. All these substances have the appearance peculiar to quartz, and show nothing that is at all analogous to that appearance which so strongly characterises calcedony; the latter, also, possesses a degree of hardness superior to that of any of the others. Besides, quartz in a concrete state is often found accidentally mixed with martial argill; a circumstance that, as is well known, frequently happens to calcedony. In the former case, a true jasper is formed, which has a quartzose base, instead of having, as in the latter case, a base of calcedony. The appearance of these two kinds of jasper is so different, that the most inexperienced eye cannot fail to distinguish them at first sight. Many other facts might be added, in support of what I have said. Calcedony, for example, is easily decomposed: there are few masses of this substance of any considerable size, few fragments of flint, jasper, &c. which have lain for any length of time upon the surface of the earth, that do not afford a demonstration of it. Most kinds of cacholong are nothing more than the effect of this decomposition, in a more or less advanced state, which (as happens in the girasol, but in a less perfect and less striking manner) causes the stones in which it takes place, to possess the property of becoming transparent in water. Quartz, whether in the most perfectly crystallized state, or in that in which its crystallization has been the most disturbed, possesses nothing which can be compared with the above property. I cannot therefore consider quartz as calcedony, properly so called; nor can I consider the substance I have distinguished

by the name of girasol, as being either quartz or calcedony. But, it will be said, what is then the nature of those substances? To this question I can only answer, I do not know; and, unfortunately, those substances are not the only ones respecting which we are obliged to confess our ignorance. We make, however, one step towards the knowledge of the nature of a substance, when we discover that it is not what it has hitherto been supposed to be. Calcedony and girasol certainly have for their base, the same earth that forms the base of quartz; but that earth appears to me to be differently modified in those substances; that is to say, I do not believe that quartz is merely an aggregate of pure quartzose earth. In like manner, I do not believe that corundum is merely an aggregate of pure argill. Perhaps, among the number of interesting discoveries with which chemistry is constantly enriching us, we shall one day be enabled to place that of a more intimate acquaintance with the nature of the above-mentioned substances.

Besides the attraction which takes place between the *similar* integrant molecules of substances, which I have already mentioned, there exists another, between these similar molecules and those which are *dissimilar*, or of a different nature. In consequence of this kind of attraction, during the formation of a substance by the aggregation of its similar molecules, other molecules of a different substance, being situated near them, enter into union with them, either by one molecule uniting with another, or by a collection of molecules uniting themselves to a molecule. To the first of these, I shall give the name of *simple homogeneous attraction of aggregation;* the other I shall call *heterogeneous attraction of aggregation;* and, to those extraneous molecules which the last mentioned kind of attraction introduces

into a substance, I shall give the name of *accidental integrant molecules.*

The effects of the last mentioned attraction, which is much weaker and much more variable than any of the others, seem to depend, in a great measure, upon the different circumstances in which the substances happen to be, at the moment of their formation. The new substance resulting from it, is not perfectly homogeneous in all its parts, and, of course, ceases to be a chemical compound, properly so called. Every thing seems to show, that the introduction of these accidental molecules into the abovementioned substances, is governed by a particular law of attraction; which acts in an uniform manner, so long as the formation of the substances under its influence takes place in the same circumstances; but which varies, when those circumstances happen to undergo any variation. Thus, for instance, in those kinds of tremolite that have the dolomite for matrix, (as in that from St. GOTHARD,) the carbonate of lime is in the proportion of $\frac{18}{100}$;* but, in those kinds which have an argillaceous matrix, it is only in the proportion of $\frac{4}{100}$. Thus also, the magnesian carbonate of lime, the chemical nature of which, as is shown by its form, is the same as that of pure carbonate of lime, admits, in consequence of the heterogeneous attraction of aggregation, the magnesian earth as a simple integrant part, and in proportions which vary, according to the circumstances in which the formation of this carbonate takes place. The magnesian carbonate of lime of Tyrol, for example, contains, according to

* Assisted by the analysis of Mr. CHENEVIX, I think I have proved what I have here asserted; and also, that the phosphorescence of the tremolite, which had been considered as one of the characters of that substance, is owing only to the particles of dolomite that are mixed with it. See *Journal des Mines*, No. 73.

Mr. KLAPROTH, $\frac{45}{100}$ of magnesia;* while, according to the same chemist, that which comes from Sweden, contains only $\frac{25}{100}$; yet both of them, most commonly, assume the primitive rhomboidal form of pure carbonate of lime.

In like manner also, a proportion of iron greater than that which makes a constituent part of the garnet, is observed in that substance; and this proportion varies considerably, according to the circumstances which governed its formation. If we take, from among the various analyses of this substance, by different chemists,t hose only which were made by Mr. VAUQUELIN, we shall find, that the red garnet of Bohemia afforded him $\frac{41}{100}$ of iron; the black garnet of the *Pic d'Eres-lids* in the Pyrenees

* This is likewise nearly the proportion of magnesia found in the magnesian carbonate of lime which is so common in Derbyshire, and in many other parts of England. For our knowledge of this substance, which had till then been confounded with the martial carbonate of lime, (pearl spar,) we are indebted to the analysis made by Mr. TENNANT. It is found in the form of small but very brilliant crystals, which belong to the primitive rhomboid of pure carbonate of lime; but these rhomboids do not appear to be subject to those variations in form so commonly met with in martial carbonate of lime. More frequently, however, it is in the form of a more or less granulated mass, which very often, upon being examined with a lens, shows itself to be a confused aggregation of the same small rhomboids. This magnesian carbonate of lime, is nearly equal in hardness to the martial carbonate. It dissolves, however, much more readily, and with a slight effervescence, in nitric acid; but does not give the same yellow colour to that acid, when it happens to contain iron, which it almost always does, though in very small proportion. Its mean specific gravity, taken on three specimens from different parts of Derbyshire, which varied very little from each other, was found to be 2823; the same carbonate from Tyrol, gave a mean specific gravity of 3053. The specific gravity of the magnesian carbonate is therefore greater than that of pure carbonate of lime; and there must surely have been some error in the operation that gave to Mr. KLAPROTH 2480, as the specific gravity of the former substance. The magnesian carbonate shows no phosphorescence when thrown upon a hot iron. I think it probable that, in the cabinets of various mineralogists, specimens of it have been erroneously placed among those of pearl spar.

$\frac{16}{100}$;* the red garnet from the same place $\frac{17}{100}$; and the yellow garnet of Corsica $\frac{10}{100}$.

Nevertheless, although the various molecules which, by means of this last-mentioned mode of attraction, unite themselves to mineral substances during their formation, do not cause any change in their chemical nature, they frequently, as I have already observed, occasion an alteration in their physical construction; and very often induce variations in such of their characters as most immediately depend upon that construction, such as, their specific gravity, their hardness, their transparency, and even (particularly in the class of stones) their colour. It is therefore necessary that the mineralogist should fix his chief attention upon this mode of attraction, in order that he may be able to understand the accidental causes of the variations to which the substances under his examination are subject. The chemist also ought always to bear in mind the existence of such causes, as they may be fairly suspected, in most of the substances whose nature he attempts to investigate by his analysis. If he neglects to do this, he will be constantly liable to confound, in the result of his operations, those products which really belong to the chemical composition of the substances he examines, with those which are foreign to it.

The foreign particles which the heterogeneous attraction of aggregation thus introduces into mineral bodies, necessarily affect their homogeneity. Yet, when that mode of attraction has taken place with all the perfection of which it is susceptible, the

* The black garnet from Frascati near Vesuvius, of which Mr. WERNER (for I know not what reason) has made a particular species, was also found by Mr. KLAPROTH to contain $\frac{16}{100}$ of iron; whereas Mr. VAUQUELIN found the proportion of iron in this kind of garnet to be as high as $\frac{25}{100}$.

interposition of these particles, in the substances into which they are admitted, is made in such a regular manner, that the homogeneity of those substances is in some measure preserved, if not with respect to single molecules with each other, at least with respect to collections of molecules with similar collections. Hence it follows, that the substance, although it may not possess that complete transparency which belongs to it when in its highest state of chemical purity, still retains that property in a very considerable degree. This is exemplified in many crystals of magnesian carbonate of lime, of martial carbonate of lime, of garnet, &c. But, for the most part, this kind of attraction, which, on account of its being more weak than any of the others, is more easily disturbed, does not admit of the forementioned regularity; in that case, the substance which was under its influence, possesses a greater or less degree of opacity.

In short, it appears, that the molecules of foreign substances, introduced into mineral bodies, in the above-mentioned manner, by the heterogeneous attraction of aggregation, do not prove any obstacle to the action of the crystalline attraction. The only effect the former mode of attraction seems to produce upon the latter, is to cause the form of the substance submitted to its influence, to approach as nearly as possible to the most simple forms, or even to the primitive one, belonging to it; and, at the same time, to render those forms more constant. Thus, the magnesian carbonate of lime, and also the martial carbonate of lime, generally assume either the form of the primitive rhomboid, or that of the lenticular rhomboid, of pure carbonate of lime. Thus also, the quartzose carbonate of lime, commonly known by the name of sandstone of Fontainebleau, constantly assumes the form of the muriatic rhomboid (named by HAUY

inverse) of the pure carbonate of lime. Lastly, in the same manner, the kind of talc called chlorite, which frequently introduces itself into the axinite, almost always occasions the latter substance to assume one of its most simple forms.

Those stones in which there exists no other cause of union between their particles than the attraction of aggregation, and which are known by the name of aggregate stones, furnish an example of the attractive force that is really exerted by the dissimilar molecules which enter into substances during their formation. In granite, for instance, the integrant parts, which, instead of being molecules, are become masses, are as dissimilar as possible. Yet, although no ingredient whatever contributes to unite them, (their union being brought about merely by the cohesion of their surfaces,) the great degree of hardness this stone possesses, and the difficulty with which its parts are separated, when it is in a perfect state, that is to say, when its texture has not been injured by any accidental cause, are both well known. This remark may be applied to various kinds of sandstone, of schistus, &c.

The different kinds of attraction here described, may, I confess, be nothing more than mere modifications of one and the same power, originally belonging to matter; but this appears to me not yet sufficiently demonstrated. Supposing it, however, to be the case, they certainly exert as much force upon mineral substances, at the time of their formation, as could be exerted by attractive forces of a really different nature.

OBSERVATIONS UPON THE DIFFERENT KINDS OF SULPHURET OF COPPER.

The triple sulphuret of lead, antimony, and copper, described by me in the former part of this Paper, cannot fail to be considered as a substance highly interesting both to mineralogists and chemists, as it serves to show the true nature of the triple combination which sulphur enters into, with lead, antimony, and copper. The difference existing in the form, the specific gravity, the hardness, and all the other external characters of this triple sulphuret, when compared with those of the gray tetraedral sulphuret of copper, seems to me to demonstrate, that the antimony and the lead which have been so frequently supposed to be constituent parts of the last-mentioned ore, are nothing more than accidental mixtures; and that, when they happen to be met with in that ore, they have been introduced merely by the heterogeneous attraction of aggregation, and are foreign to its substance. The same opinion may, I think, be fairly adopted, respecting the silver which is sometimes found in it. If these metals were really contituent principles, how can we suppose, that the presence or the absence of one or more of them, and the great difference that exists in their proportions, (as is shown by the various analyses which have been made,) should occasion no variation whatever in the form of the sulphuret. Such a circumstance would be in direct opposition to every observation that has hitherto been made on the subject.

Having been, for a long time past, impressed with an idea, that iron and copper are the only metals combined with the sulphur, in the natural composition of the gray tetraedral sulphuret

of copper, and wishing to have the truth of this opinion ascertained by chemical analysis, I desired Mr. Chenevix, about two years ago, to be so good as to analyse seven different specimens of the above-mentioned ore, in which I suspected the presence of antimony. I requested him, at the same time, to search with every possible care, for any metal the ore might contain besides copper and iron. Of these seven specimens, one came from Kapnick in Transylvania, one from Merkirch in Alsace, one from Andreasberg in the Hartz, one from Grossmandorf in Saxony, one from Freyberg in Saxony, one from Hesse, and one from the Alps of Dauphiny. Not one of them contained a particle of lead, or of silver; but every one of them contained antimony, although in such various proportions, as to exhibit the following differences; *viz.* $\frac{38}{100}$, $\frac{28}{100}$, $\frac{17}{100}$, $\frac{10}{100}$, and $\frac{5}{100}$. From these results, (whatever error may be supposed to have been committed in the operation,) no one, I think, can possibly consider antimony as really forming a constituent part of this sulphuret. Two of the above-mentioned specimens possessed a determinate crystalline form, namely, a tetraedron. One of them, which came from Kapnick, contained $\frac{28}{100}$ of antimony; the other, which came from Hesse, contained $\frac{10}{100}$.

In order to complete this investigation, after having given Mr. Chenevix some other specimens of this gray sulphuret of copper, which had no appearance of crystallization, and which he found to contain nothing but sulphur, copper, and iron, without any trace whatever of antimony, I desired him to be so good as to add to his analyses, that of some very brilliant crystals of this same sulphuret, which came from Cornwall, and which were in the form of a tetraedron, with the edges doubly bevelled, and the solid angles truncated. They also were found to contain

nothing but copper, iron, and sulphur, in the following proportions; namely, $\frac{52}{100}$ of copper, $\frac{33}{100}$ of iron, $\frac{14}{100}$ of sulphur. Perhaps we may, with some reason, consider the latter analysis, as that of this sulphuret in its purest state, and consequently as that which most decidedly declares the true nature of the tetraedral gray martial sulphuret of copper, when free from those extraneous metals that so frequently unite with it, by introducing themselves within its substance.

This gray copper ore (the *Fablerz* of the Germans) is therefore, in my opinion, shown to be nothing more than a simple combination of sulphur with copper and iron, in other words, a double sulphuret, of copper and iron; but it is very apt, during its formation, to admit other metallic substances into its composition, by the heterogeneous attraction of aggregation. When this double sulphuret is scratched with a knife, the powder obtained is sometimes of a black colour: this is always the case when the sulphuret is unmixed. At other times, the powder is of a reddish-brown colour; and it may then be presumed, that the sulphuret contains a mixture of silver and antimony, generally combined together, in the state of red silver.

I have seen many specimens of this substance, in which, by means of a lens, particles of red silver might be perceived. In some specimens, they might be seen with the naked eye. When this sulphuret is taken from a mine that contains sulphuret of lead, it very frequently contains some particles of the latter metal.

On the other hand, however, I cannot consider, as most mineralogists do at present, yellow copper ore (the *Kupferkies* of the Germans) as a mere martial pyrites, or sulphuret of iron, holding copper interposed within its substance. This ore also

appears to me to be a double sulphuret, of copper and iron; but constituting a species distinct from the gray sulphuret of the same form. Chemistry has not yet ascertained, in a certain and satisfactory manner, the proportions of the constituent parts of this yellow double sulphuret. Its primitive form is a regular tetraedron, of which the octaedral form it sometimes exhibits, is only a modification, produced by each of its solid angles having been replaced by a plane, which is perpendicular to the axis passing through these angles. That this is really the case, is also proved, by my having seen specimens of this double sulphuret, (and chiefly among those that came from Cornwall,) which exhibited several of the well known modifications of the regular tetraedron; a circumstance that never takes place in the octaedral sulphuret of iron, even when it happens accidentally to contain a portion of copper.*

* Though I do not admit that the above-mentioned yellow copper ore, is merely a sulphuret of iron, with copper interposed within its substance, I am far from asserting, that the last mentioned sulphuret does not sometimes contain a portion of copper interposed within it; but, when that happens, the copper is generally in much smaller quantity, and its proportions are very irregular, insomuch that, in a hundred weight of sulphuret of iron containing copper, the quantity of copper varies, from a few ounces to several pounds. Besides, this sulphuret of iron, in the above-mentioned circumstances, constantly preserves the external characters that are peculiar to it; which, as we shall hereafter see, are totally different from those of the yellow double sulphuret.

But, what appears to me worthy of remark is, that when this sulphuret of iron, thus mixed with copper, assumes a determinate form, that form is always one of those belonging to the octaedral sulphuret of iron; whereas, when gold happens to be, in the above manner, interposed within the substance of this sulphuret, it is always in that kind which assumes the form of striated cubes, or in that dodecaedral modification of the above form which has pentagonal planes.

The constancy of the above facts, sufficiently shows that they are not owing to

Among the above forms, there is one in particular, which has not yet been described as belonging to this sulphuret; namely, the dodecaedron with rhombic planes, and also the passage of the tetraedron, more or less advanced, towards this dodecaedron, by each of its solid angles having been replaced by three planes, situated upon its sides. This dodecaedron, which is by no means common, even in Cornwall, (the only place where I have yet met with it,) is found there of a pretty considerable size, being sometimes an inch, or even more, in diameter. This form has never yet been observed among the sulphurets of iron.

All the other characters of the tetraedral yellow double sulphuret, are likewise different from those of the octaedral sulphuret of iron. Its yellow colour is more deep; its fracture is more brilliant; its grain is much less even, and exhibits some

chance. In like manner, as those sulphurets of iron which contain neither copper nor gold, are found in the form of octaedrons, of striated cubes, and of dodecaedrons, it is evident that the form of such sulphurets is not modified by either of the last-mentioned metals.

The above observations, in my opinion, tend very much to confirm what I have advanced with respect to the heterogeneous attraction of aggregation, which appears (in an infinite number of circumstances) to take place between the integrant molecules of one substance and others that are of a nature totally different, and gives rise to a body that is physically, but not chemically, different from what would have been produced, if these heterogeneous molecules had not been interposed within it.

Thus, even the sulphuret of iron generally contains a much larger proportion of sulphur than that which combines with the iron, during its formation. This superfluous proportion of sulphur is consequently foreign to the nature of the sulphuret, and is not necessary to its formation. It may be separated by means of distillation, without decomposing the real sulphuret; and manifests itself in a very striking manner, when, after having grossly powdered the sulphuret, a portion of the powder is thrown upon a live coal, or a red hot iron, by the inflammation which takes place, on account of this portion of the sulphur being in an uncombined state. All kinds of pyrites, when treated in this manner, emit a phosphorescent light, of a fiery red colour.

parts that are smooth, and sometimes partially conchoid; which appearances are never observed in the octaedral sulphuret of iron. The hardness of this double sulphuret is also less considerable. The octaedral sulphuret of iron scratches it with great ease; and, if we endeavour to obtain sparks from it, by means of a piece of steel, it is with great difficulty that any can be procured: it is also more brittle. If grossly powdered, and thrown upon an iron heated to redness, although it then emits a strong smell of sulphureous acid, we do not perceive that inflammation of the uncombined sulphur which, as I have already said, takes place when the octaedral sulphuret of iron is treated in the same manner; yet this last-mentioned kind of sulphuret, and that in striated cubes, are those which contain the smallest quantity of superabundant or uncombined sulphur. The powder of the yellow double sulphuret, however, when thrown upon the hot iron, emits a fiery red light, similar to that which proceeds from the sulphuret of iron; indeed its light is still more vivid. Lastly, its specific gravity is less considerable; that which I obtained from a trial of several crystals, of a perfectly determinate form, was always between 4000 and 4100; the mean being 4058. Whereas, the specific gravity of the octaedral sulphuret, taken in similar circumstances, was between 4900 and 5000; the mean being 4944.

But the yellow double sulphuret, notwithstanding it exhibits, or at least seems to exhibit, the same primitive form as the gray tetraedral sulphuret, (although it is far from admitting its various modifications,) seems to be by no means of the same nature with it, and ought, in my opinion, to be considered as forming a species which, though analogous to the other, is really different from it. The colour (a character of

infinitely more consequence in metals than in stones) of the first-mentioned sulphuret is always a bright deep yellow, whereas that of the other is a blackish gray; this circumstance alone would be sufficient to create some doubts respecting the identity of these two substances, but their other characters also present very striking differences. The gray sulphuret is harder: its powder, instead of being of a greenish-brown colour, like that of the yellow sulphuret, is black. This powder, when thrown upon an iron heated to redness, emits neither the smell of sulphureous acid, nor the beautiful phosphorescent light of the other. The specific gravity of the gray sulphuret, taken from crystals of a perfectly determinate form, was always found to be between 4460 and 4560, the mean being 4512; while that of the yellow double sulphuret is, as I have already stated, 4058. The crystal that had the highest specific gravity came from Cornwall, and belongs to the kind I have already described as containing only copper and iron combined with the sulphur; which kind, I think, ought to be considered as a standard of comparison in this species of double sulphuret. The specific gravity of the above crystal was 4558.

A question here naturally presents itself, to which, if we consider the present state of our knowledge, it appears not very easy to furnish an answer. As the true sulphuret of copper is of a blackish-gray colour, and the tetraedral gray double sulphuret (*Fahlerz*) is also of that colour, how happens it, that the yellow double sulphuret (*Kupferkies*) has always that brilliant yellow colour which characterises it, and which is at the same time the principal cause that leads many mineralogists to consider it as being nothing more than a martial pyrites mixed with copper? To answer this question, as I

have already said, appears to me by no means easy. Is it possible that the iron, which Mr. PROUST, in his Memoir upon metallic Sulphurets, has shown to be in a metallic state in the sulphuret of iron, is also in a metallic state in the yellow double sulphuret, and is, on the contrary, in the state of oxide in the gray double sulphuret? Or, can the property of emitting a beautiful phosphorescent fiery light, when thrown upon a heated iron, (which property is common both to the yellow double sulphuret, and to the martial pyrites,) have any connection with the cause upon which the colour depends? It is true that Mr. PROUST, in the memoir already referred to, says that the sulphuret of copper, when in its state of greatest purity, is of a deep blue colour, with a coppery appearance: and he states this colour to be one of the characters by which that substance may be distinguished. This observation may perhaps be true, with respect to the artificial sulphuret of copper, for I am not acquainted with that substance; but it would certainly lead astray any naturalist who should attempt to use it as a guide to distinguish the natural sulphuret of copper. The true colour of the latter substance, when in its most pure state, is a very dark gray. In that state, it assumes a peculiar crystalline form, and, when cut with a knife, exhibits a metallic lustre, as is the case with sulphuret of silver.

A portion of iron sometimes combines with the sulphuret of copper, and produces a new kind of double sulphuret, which really exhibits the colours mentioned by Mr. PROUST, and particularly the reddish colour of copper, or of nickel: it is the *Buntkupfererz* of WERNER. This new kind of double sulphuret also crystallizes, and in forms which are peculiar to it, and which are not at all analogous to those of the other double sul-

phurets; among these forms, however, the octaedron (the only form mentioned by Mr. WERNER) appears to me not to exist. To this species belongs that which, in Mr. KLAPROTH's analysis, afforded him $\frac{50}{100}$ of copper, $\frac{25}{100}$ of iron, and $\frac{20}{100}$ of sulphur; and which Mr. WERNER makes his second sub-species of vitreous copper, under the denomination of *lamellated vitreous copper;* but it is in fact a variety of the *Buntkupfererz,* not of the vitreous copper. It is true indeed, that this latter sometimes contains a portion of iron, but always a very inconsiderable one, within its substance; and, when that happens, the iron is totally foreign to its composition. Among the analyses of the various combinations of copper with sulphur, which Mr. CHENEVIX was so good as to make at my request, (the specimens for that purpose having been furnished by me,) one was that of a very pure sulphuret of copper, which came from Cornwall, and was in crystals of a perfectly determinate form. The constituent parts of this sulphuret appeared, by the analysis, to be $\frac{81}{100}$ of copper, and $\frac{19}{100}$ of sulphur. In six others of the above-mentioned specimens, there seemed to be a mixture of iron, varying in proportion, from $\frac{3}{100}$ to $\frac{6}{100}$. Lastly, from the analysis of several specimens of the sulphuret last described, (*Buntkupfererz,*) which were of the colour of nickel, I am induced to believe that the proportions of the real constituent parts of this species, must be very nearly as follows, *viz.* from $\frac{60}{100}$ to $\frac{65}{100}$ of copper, and from $\frac{15}{100}$ to $\frac{18}{100}$ of iron, the remainder being sulphur.

From what I have here observed, which however is to be considered merely as a cursory account of the various sulphurets of copper, it may easily be inferred, that there exist many species of this substance, which have not yet been

described; also, that several of those with which we are acquainted, have not been sufficiently examined. These sulphurets are well worthy of fixing the attention of chemists. The observations to which they may give rise, are sufficiently interesting to engage their attention, and induce them to bestow particular care on their inquiries concerning them, as such inquiries may perhaps tend to increase our knowledge respecting the nature of copper, and also of iron. But I cannot too strongly recommend to those chemists who may engage in the research, to be very particular in the choice of the specimens they make use of, and also to make a great number of experiments, that they may serve as objects of comparison to each other.

Having very favourable opportunities of examining, and comparing with each other, the different kinds of copper ore that are found in the county of Cornwall, (which county furnishes a greater variety of combinations of that metal than any other part of Europe, several of which are peculiar to it,) it was impossible that my attention should not be attracted by the many interesting facts that came under my observation. Next to the combinations of this metal with the arsenical acid, the study of its sulphurets has been the principal object of my pursuits; and, if future circumstances should concur with my wishes in that respect, I hope I shall have it in my power to present to the public the result of my observations on those sulphurets.

Those observations will, I trust, furnish additional proofs of the truth of a circumstance respecting which I have long ceased to have any doubts, and which I have already, on various occasions, (particularly in my description of the arseniates of copper,) attempted to establish, namely, that there exist many minerals, which differ in species, although they have the same

substance, or collection of substances, for their basis, and are combined with the same modifying substance; and that the difference between these minerals, arises merely from the different proportions of their basis, and of their modifying substance. The more I study the works of nature, the more I become persuaded of the truth of the above remark; I am also convinced, that a want of attention to this circumstance has led mineralogists to confound one species with another.

If to the above cause of error we add that which is occasioned by neglecting, in the analysis of these substances, to distinguish those parts which (by their chemical combination and mode of attraction) determine the particular nature of the substance analysed, from those which enter into its formation in consequence of the heterogeneous attraction of aggregation, we shall readily conceive that it is the duty of chemists to correct gradually the errors they have occasioned in mineralogy. But I must repeat that, in order to correct these errors, it is necessary to make a number of comparative analyses of the substance whose nature they wish to determine. It is also necessary, that the specimens they make use of should be very perfect in their kind, taken from various districts, and, as much as possible, from various matrices. It may perhaps not be improper to add, that the interest of science requires, that the mineralogist and the chemist should mutually sanction the operations each of them, in his respective department, undertakes to perform.

Fig. 1.

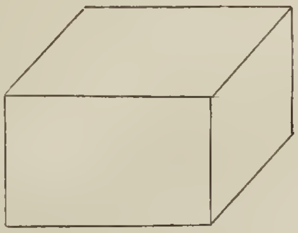


Fig. 2.

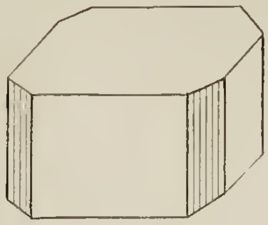


Fig. 3.

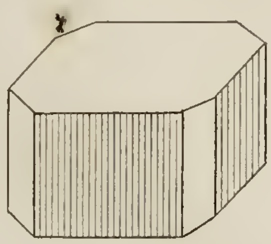
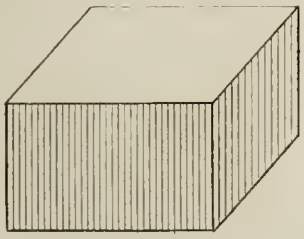


Fig. 4.



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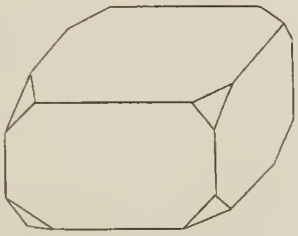

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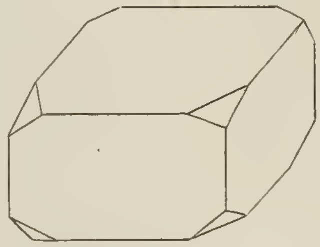

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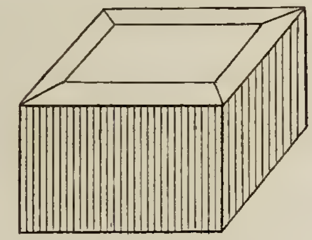


Fig. 8.

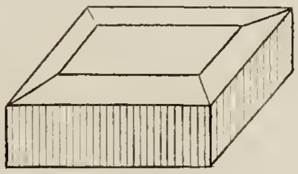


Fig. 9.

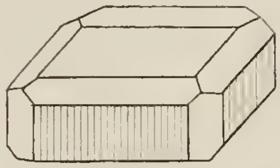


Fig. 10.

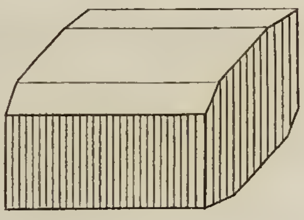
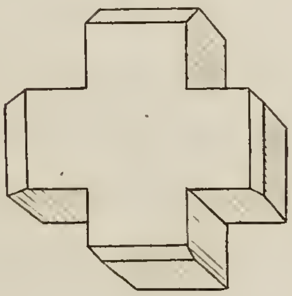


Fig. 11.



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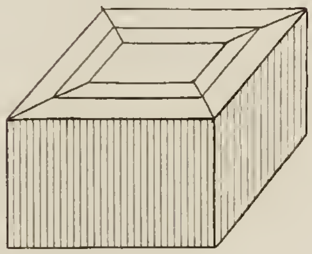


Fig. 13.

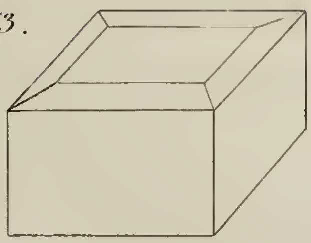


Fig. 14.

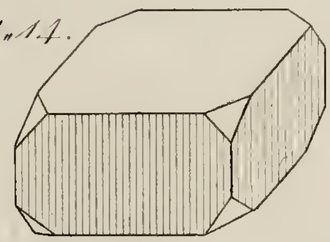


Fig. 15.

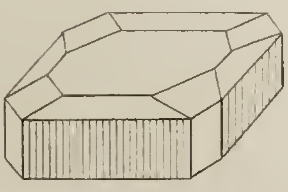


Fig. 16.

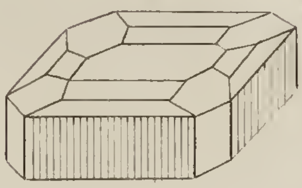
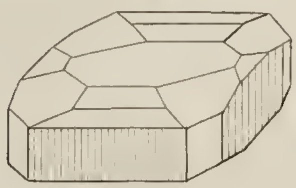


Fig. 17.



Basire sc.

V. *Analysis of a triple Sulphuret, of Lead, Antimony, and Copper, from Cornwall. By* Charles Hatchett, *Esq. F. R. S.*

Read January 26, 1804.

The substance which forms the subject of this Paper, has hitherto been regarded as an ore of antimony; it is extremely rare, and has only been obtained from Huel Boys, in the parish of Endellion, a mine which, from deficiency of profit, has for some time been abandoned.

The scarcity of the ore has probably been the cause of its being unknown to foreign mineralogists; indeed few even of the British cabinets possess it; but the most perfect and beautiful specimens are (as far as I know) to be seen in the splendid collection of Philip Rashleigh, Esq. of Menabilly, in Cornwall.

To Mr. Rashleigh we are indebted for the first description of this ore;* but no subsequent notice had been taken of it, until the preceding Paper was written by the Count de Bournon, whose eminent merits, as a mineralogist and crystallographer, are well known to this Society.

1.

The specific gravity of this substance is 5,766, at 65° of Fahrenheit.

* Specimens of British Minerals, selected from the Cabinet of Philip Rashleigh, Esq. F. R. S. &c. Part I. page 34, Plate XIX.

II.

If suddenly heated on charcoal, by the blowpipe, it crackles and splits; but, when gradually exposed to the flame, it liquefies, and, upon cooling, assumes a dull metallic gray colour.

When the globule was longer exposed to heat, white fumes (which at first had a sulphureous odour) were evolved, and partly settled on the charcoal.

Ebullition prevailed during the discharge of these white fumes; and the globule gradually suffered considerable diminution, remaining at length tranquil, and of a very dark gray colour.

Upon examination, this appeared to be principally sulphuret of lead, which, like a crust, enveloped a minute globule of metallic copper, so malleable as to bear to be flattened by a hammer.

III.

Some of the ore, finely powdered, was put into a matrass, and nitric acid diluted with an equal portion of water was poured on it. Upon being digested in a low heat, a considerable part was dissolved, with much effervescence. Some sulphur, which floated, was separated; and the clear liquor, which was bluish green, was decanted from the residuum at the bottom of the vessel.

A great part of the excess of acid being expelled from the solution, it was largely diluted with distilled water, and some dissolved muriate of soda was added; but this did not produce any alteration in the transparency of the liquor. A solution of sulphate of soda was then poured in, and formed a very copious precipitate of sulphate of lead.

When this had been separated, the liquor was saturated with ammonia; by which it was changed to a deep blue colour. A

few flocculi of iron were separated; and the remainder was found to contain nothing but copper.

The sulphur which had floated, was added to the residuum which had subsided to the bottom of the matrass; and the whole was digested with muriatic acid. This solution was of a straw colour; and, when separated from the sulphur, and poured into a large quantity of water, afforded a plentiful white precipitate.

This precipitate was completely resolved into white fumes, by the blowpipe; and the muriatic solution of it, when added to water impregnated with hydro-sulphuret of ammonia, formed the orange coloured precipitate, commonly known by the appellation of golden sulphur of antimony.

IV.

Muriatic acid did not immediately act upon the pulverized ore; but a solution was speedily effected by the addition of a few drops of nitric acid: pure sulphur was separated; and the liquor, being decanted into water, yielded a copious precipitate of oxide of antimony.

The filtrated solution, by gradual evaporation, afforded crystals of muriate of lead; and the lead which afterwards remained in the liquor, was separated by a few drops of sulphuric acid.

The solution was now of a bright green colour, and, as before, was found only to contain copper, and a minute portion of iron; the former was therefore precipitated in the metallic state, by a plate of zinc.

These experiments, with others which I have not thought necessary to mention, prove, that the constituent parts of this ore are lead, antimony, copper, and a little iron, combined with

sulphur; and, when the specific gravity, the external and internal colour, fracture, grain, and other characters are considered, there can be no doubt but that at least the three first metals exist in the ore, in, or nearly in, the metallic state, combined with sulphur, so as to form a triple sulphuret; to ascertain the proportions of which, the following analysis was made.

V.

ANALYSIS.

A. 200 grains of the ore, reduced to a fine powder, were put into a glass matrass, and, two ounces of muriatic acid being added, the vessel was placed in a sand-bath. As this acid, even when heated, scarcely produced any effect, some nitric acid was gradually added, by drops, until a moderate effervescence began to appear.

The whole was then digested in a gentle heat, during one hour; and a green coloured solution was formed, whilst a quantity of sulphur floated on the surface, which was collected, and was again digested in another vessel, with half an ounce of muriatic acid.

The sulphur then appeared to be pure, and, being well washed and dried on bibulous paper, weighed 34 grains: it was afterwards burned in a porcelain cup, without leaving any other residuum than a slight dark stain.

B. The green solution, by cooling, had deposited a white saline sediment; but this disappeared upon the application of heat, and the addition of the muriatic acid in which the sulphur had been digested.

The solution was perfectly transparent, and of a yellowish

green: it was made to boil, and in this state was added to three quarts of boiling distilled water, which immediately became like milk; this was poured on a very bibulous filter, so that the liquor passed through before it had time to cool; and the white precipitate thus collected, being well edulcorated with boiling water, and dried on a sand-bath, weighed 63 grains.

C. The washings were added to the filtrated liquor; and the whole was gradually evaporated at different times, between each of which it was suffered to cool, and remain undisturbed during several hours. A quantity of crystallized muriate of lead was thus obtained, until nearly the whole of the liquor was evaporated: to this last portion a few drops of sulphuric acid were added, and the evaporation was carried on to dryness; after which, the residuum, being dissolved in boiling distilled water, left a small portion of sulphate of lead.

The crystallized muriate of lead was then dissolved in boiling water; and, being precipitated by sulphate of soda, was added to the former portion, was washed, dried on a sand-bath, and then weighed 120.20 grains.

D. The filtrated liquor was now of a pale bluish-green, which changed to deep blue, upon the addition of ammonia; some ochraceous flocculi were collected, and, when dry, were heated with wax in a porcelain crucible, by which they became completely attractable by the magnet, and weighed 2.40 grains.

E. The clear blue liquor was evaporated nearly to dryness; and, being boiled with strong lixivium of pure potash, until the whole was almost reduced to a dry mass, it was dissolved in boiling distilled water; and the black oxide of copper, being collected and washed on a filter, was completely dried, and weighed 32 grains.

200 grains of the ore, treated as here stated, afforded,

		Grains.
A.	Sulphur - - -	34.
B.	Oxide of antimony -	63.
C.	Sulphate of lead - -	120.20
D.	Iron - - -	2.40
E.	Black oxide of copper -	32.

But the metals composing this triple sulphuret are evidently in the metallic state; and white oxide of antimony precipitated from muriatic acid by water, is to metallic antimony as 130 to 100; therefore, the 63 grains of the oxide must be estimated at 48.46, grains of the metal.

Again, sulphate of lead is to metallic lead as 141 to 100; therefore, 120.20 grains of the former are = 85.24 grains of the latter. And, lastly, black oxide of copper contains 20 *per cent.* of oxygen; consequently, 32 grains of the black oxide are = 25.60 grains of metallic copper.

The proportions for 200 grains of the ore, will therefore be,

Sulphur - - -	34.
Antimony - -	48.46
Lead - - -	85.24
Iron - - - -	2.40
Copper - - -	25.60
	195.70
Loss -	4.30

Or, *per cent.*

Sulphur - - -	17.
Antimony - -	24.23
Lead - - -	42.62
Iron - - -	1.20
Copper - - -	12.80
	97.85
Loss -	2.15

These proportions, I have reason to believe, are tolerably exact; for I did not observe any essential variation in the results of two other analyses, which I made of this substance, with every possible precaution.

The loss may be principally ascribed to the oxide of antimony and sulphate of lead; but especially to the former, which has a great tendency to adhere to filters and glass vessels.

In some of the preliminary experiments, I obtained a small portion of zinc; but, having received, through the kindness of Mr. R. Phillips, of Lombard-street, some pure crystals of the ore, I found that the zinc had proceeded from blende, which was imperceptibly mixed in the specimens which I had first examined.

VI. *Observations on the Orifices found in certain poisonous Snakes, situated between the Nostril and the Eye. By* Patrick Russell, *M. D. F. R. S. With some Remarks on the Structure of those Orifices; and the Description of a Bag connected with the Eye, met with in the same Snakes. By* Everard Home, *Esq. F. R. S.*

Read February 2, 1804.

In the description of the *Fer-de-lance* or yellow snake of Martinico, the Count de la Cepede has remarked an orifice on each side of the head, between the nostril and the eye, which had by some naturalists been conceived to be the external organ of hearing; but, not having an opportunity himself to ascertain the fact by dissection, he recommends it as an interesting object of future investigation.*

I have, in the course of the last three years, received two colubers from Java; and, by favour of Dr. Clark, two from Martinico; all four venomous, and distinguished by lateral orifices. In the month of January, 1803, Dr. Garthshore presented me with a specimen of the yellow snake of Martinico, in excellent preservation.

Six subjects, distinguished by these lateral orifices, now in my possession, offering a fair opportunity to determine a curious circumstance in comparative anatomy, the specimens were submitted to my friend Mr. Home, of whose assistance I had more than once availed myself, in similar investigations. My request

* *Hist. Nat.* Tom. II. p. 122.

was once more attended to; and the subjoined description and remarks were received in return.

Among the specimens submitted to Mr. HOME, was one of the *Bodroo Pam*, in the description of which, lately published,* I have misrepresented the orifices now in question as the nostrils, having entirely overlooked the real nostrils.

While the anatomical disquisitions were going on, inspection was made into some of the numerous collections of serpents preserved in the museums in London. In the British Museum I was shown, exclusive of the rattle-snake and the *Fer-de-lance*, four or five colubers † with lateral orifices; in the LEVERIAN Museum, I found two or three; in the HUNTERIAN Museum, two colubers,‡ and three boæ; § and in that of Mr. HEAVISIDE, one coluber. ||

The total found in the museums above-mentioned, (exclusive of the rattle-snake,) were ten or eleven colubers, and three boæ; which, added to five colubers in my own possession, amount to eighteen or nineteen subjects furnished with lateral orifices.

It appears, on the whole, that the lateral orifices have hitherto been found only in venomous serpents.

That (exclusive of the rattle-snake) they have been found in fifteen or sixteen species of colubers, and in three of the genus boa.

That they have not as yet been discovered in any of the genus anguis.

Mr. HOME'S investigations have clearly established, that these

* Account of Indian Serpents collected on the Coast of Coromandel, No. IX. Lond. 1796.

† All, I believe, non-descripts.

‡ No. 977, 1058.

§ No. 893, 1016, 1046.

|| No. 64.

lateral orifices in serpents, and the bags to which they lead, have no communication with the organ of hearing. Another fact ascertained by him is, that serpents distinguished by lateral orifices, have a cavity situated between the bag and the eye, which, so far as I know, has not been observed before.

Mr. Home's *Description, and Remarks.*

The orifices situated between the eye and the nostril, in the rattle-snake, and in some species of coluber, do not lead to the nostril or to the ear, but to a distinct bag, of a rounded form; there is a hollow of the same shape surrounded by bone, and adapted to receive it. Dr. Tyson's description of the rattle-snake is tolerably accurate: he says, "between the nostrils and "the eyes, but somewhat lower, were two orifices, which I took "for the ears; but after, I found they only led into a bone, that "had a pretty large cavity, but no perforation."*

The cavity which Dr. Tyson describes to be in the bone, is a cup, formed by the bones of the skull and those of the upper jaw; it is in shape not unlike the orbit, and is formed in a similar manner.

These bags bear a relative proportion to the size of the snake; they are lined, as also the eyelids, with a cuticle, which forms the transparent cornea, making a part of the outer cuticle, and is shed with it; and, when examined after the snake has cast it off, their shape is more perfectly seen than under any other circumstances.

In the annexed figures, one of these bags is represented in different views; all of them of the natural size, both in the *Fer-de-lance* or yellow snake of Martinico, and in the detached

* Phil. Trans. Vol. XIII. p. 26.

cuticle of the rattle-snake. The appearance in the *Bodroo Pam* is exactly the same; but, as the bag in that snake is of a smaller size, it was considered unnecessary to give a representation of it.

In the deer and antelope there are bags, in the same relative situation respecting the eye and the nose, resting upon the skull; there is also a cavity in the bone, adapted to receive them. The bags vary in size in the different species of these genera. The French naturalists have given the name of *larmiers* to these bags, conceiving them to be receptacles for the tears, of which the thinner parts evaporating, a substance remains called *larmes de cerf.*

I requested my friend Mr. ANDRE to examine these bags in the common buck, and to observe their relative position to the puncta lachrymalia; his situation in the Earl of EGREMONT's family, at Petworth, affording him every opportunity for doing it. He informs me, that the bags are lined with a cuticle, similar to that of the meatus auditorius externus in the human ear; their internal surface is smooth, free from hair, and without any appearance of glandular structure. From the inner angle of the eye to this bag, there is a kind of gutter in the skin, of a darker colour than the rest of the skin in light coloured animals, and the hairs are shorter than on the rest of the body: the substance contained in the bags resembled that found in the ears.

The lachrymal gland in the deer, he says, is very large, and the puncta so much so, as to admit the rounded end of a common probe. There is no lachrymal sac; the tubes from the puncta unite, and pass through a small opening in the bone, to the nose.

The following account of these bags, in the antelope of Sumatra, was transmitted to me in the year 1792, by Mr. BELL.

" The external orifice is of the size of a crow-quill; it leads into " a bag not larger than a small marble, which is lined with a " cuticle, with hair. From this bag there is a secretion of a " limpid fluid, which keeps oozing down the nose." This gentleman, unfortunately for natural history, died at Sumatra, soon after the date of his letter.

In the Hunterian Museum, intrusted by government to the care of the College of Surgeons, there are several specimens of these bags, from the Egyptian antelope with annulated horns, and also from some other species: these are preserved so as to show the internal cavity of the bag, and the structure of the gland immediately behind it. In these specimens, the glandular part is $\frac{1}{4}$ of an inch in thickness; from the centre of this gland, an excretory duct opens into the bag, immediately opposite to the external orifice. The bag itself is lined with a cuticle, and thinly set with strong hairs.

The facts now produced are sufficient to prove that these bags have a secretion of their own, the quantity of which varies, according to the climate and other circumstances; and there is no reason for thinking that the tears ever pass into them, the passage into the nose being unusually free, and the orifices in the bags, in many species, unfavourably situated for the reception of the tears.

We are at present unacquainted with the use to which the fluid secreted in these bags is applied.

As amphibious animals, in general, have no glands to supply the skin with moisture from within, but receive it by coming in contact with moist substances, it is possible the bags, in the snake, may be supplied in that manner, and the more so, as the cuticular lining appears perfect.

Another peculiarity is remarkable in snakes furnished with the bags described above, namely, an oval cavity, situated between the bag and the eye, the opening into which is within the inner angle of the eyelid, and directed towards the cornea. In this opening there are two rows of projections, which appear to form an orifice, capable of dilatation and contraction. From the situation of these oval cavities, they must be considered as reservoirs for a fluid, which is occasionally to be spread over the cornea; and they may be filled by the falling of the dew, or the moisture shaken off from the grass through which the snake passes.

This apparatus in the snake, has that position which is best adapted to pour out the fluid upon the cornea, when the head of the snake is erect.

Dr. Tyson had superficially observed the apparatus which has been described, and considered it as a membrana nictitans. He says, "inwards it seemed to have a membrana nictitans, "which removes any dust that might adhere to the eye."*

As snakes in general have no apparatus to wash the cornea, these particular species must have some peculiarities in their mode of life, with which we are not at present acquainted.

* Phil. Trans. Vol. XIII. p. 27.

Explanation of the Figures. See Plate III.

Fig. 1. Represents a side view of the head of the *Fer-de-lance* or yellow snake of Martinico, to show the external appearance of the orifice, with its relative situation to the nostril and the eye. The parts are delineated of their natural size.

Fig. 2. A side view of the head of the same snake, in which the bag is laid open. At the aperture of the cavity, which opens towards the cornea, there is a double row of small projecting points.

Fig. 3. The cuticle of the rattle-snake, after it had been cast off from one side of the head, represented of its natural dimensions. In this view, the internal surface only of the cuticle is seen. There is an aperture, of an irregularly oval form, which is the opening of the nostril: a little farther on is the lining of the rounded bag, in a distended state; nearer the eye is the cavity communicating with the space before the cornea, it is of an oval form, and has a narrow neck; beyond this neck is the transparent cornea, which in the snake is cuticular, and is shed with the external covering of the other parts. Through the transparent cornea, a bristle is seen passing before its external surface into the cavity.

This figure is taken from a preparation in the Hunterian Museum.

Fig. 1.

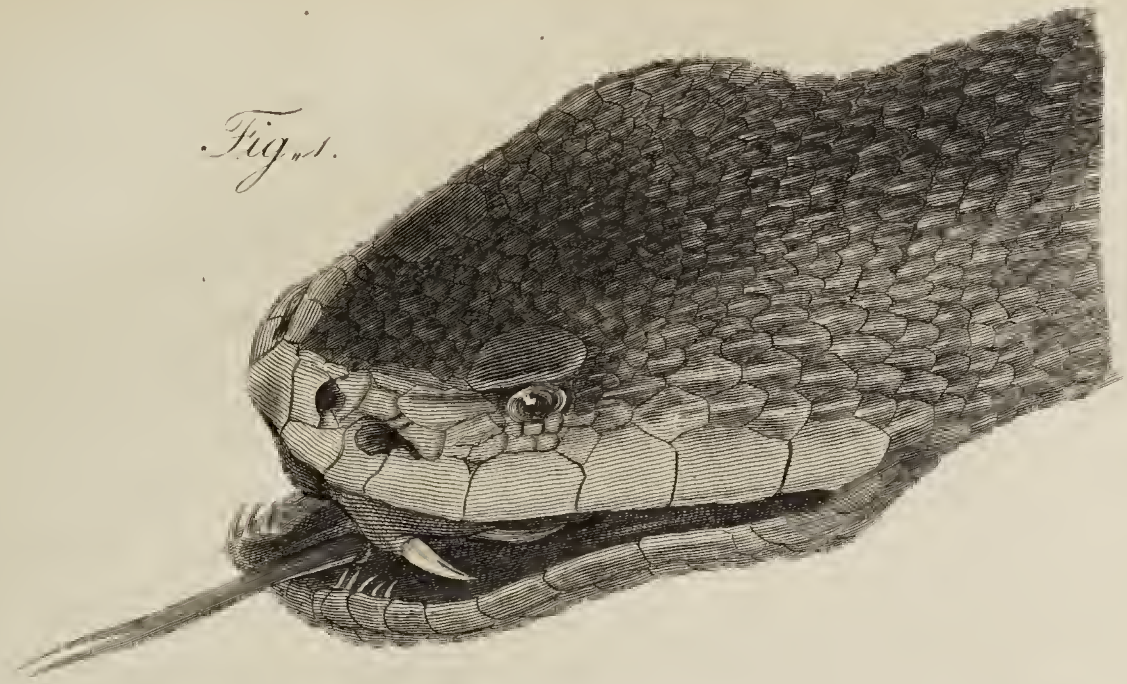
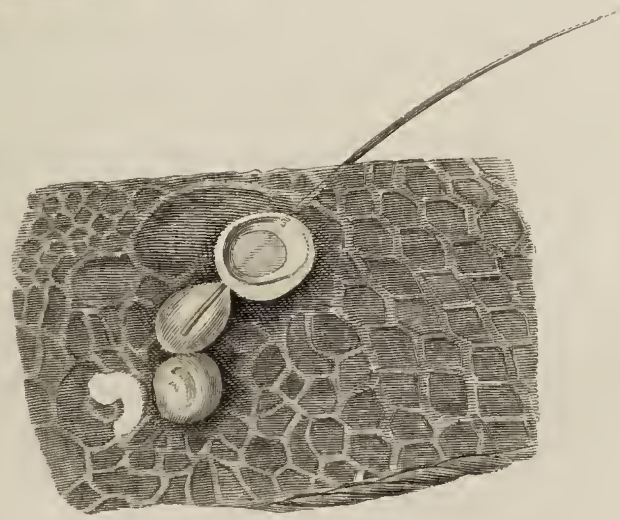


Fig. 2.

Fig. 3.



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VII. *An Enquiry concerning the Nature of Heat, and the Mode of its Communication. By* Benjamin *Count of* Rumford, *V. P. R. S. Foreign Associate of the National Institute of France, &c.*

Read February 2, 1804.

HEAT is employed in such a vast variety of different processes, in the affairs of life, that every new discovery relative to it must necessarily be of real importance to mankind; for, by obtaining a more intimate knowledge of its nature and mode of action, we shall no doubt be enabled not only to excite it with greater economy, but also to confine it with greater facility, and direct its operations with more precision and effect.

Having many years ago found reason to conclude, that a careful observation of the phenomena which attend the heating and cooling of bodies, or the communication of heat from one body to another, would afford the best chance of acquiring a farther insight into the nature of heat, my view, in all my researches on this subject, has been principally directed to that point; and the experiments of which I am now to give an account, may be considered as a continuation of those I have already, at different times, had the honour of laying before the Royal Society, and of presenting to the public in my Essays.

In order that the attention of the Society may not be interrupted unnecessarily, by descriptions of instruments, in the midst of the accounts of interesting experiments, I shall begin by describing the apparatus which was provided for these researches;

and, as a perfect knowledge of the instruments made use of, is indispensably necessary, in order to form distinct ideas of the experiments, I shall take the liberty to be very particular in these descriptions.

The thermometers, four in number, which were used in these experiments, were constructed under my own eye, and with the greatest possible care; and, after every trial I have been able to make with them, in order to ascertain their accuracy, they appear to be very perfect.

They are mercurial thermometers, graduated according to FAHRENHEIT: their bulbs are cylindrical, 4 inches long, and $\frac{4}{10}$ of an inch in diameter; and their tubes are from 15 to 16 inches long. The mercury with which they are filled is quite pure; and they are freed from air. Their scales were divided with the greatest care; and, by means of a nonius, they show eighth parts of a degree very distinctly: they are graduated from about 10 degrees below the freezing point, to 5 or 6 degrees above the point of boiling water. Their bulbs are quite naked; their scales ending about 1 inch above the junction of the bulb with its tube. The freezing point is situated about 5 inches above the upper end of the bulb. The reason for placing it so high, will be evident, from the details of the experiments in which these instruments were used.

The instrument I contrived for ascertaining the warmth of clothing, is extremely simple: it is merely a hollow cylindrical vessel, made of thin sheet brass. It is closed at both ends; and has a narrow cylindrical neck, by which it is occasionally filled with hot water.

This vessel, being covered with a garment made to fit it,

composed of any kind of cloth, or stuff, or other warm covering, is supported, in a vertical position, on a wooden stand, which is placed on a table, in a large quiet room; and, one of the thermometers above described being placed in the axis of the vessel, the time employed in cooling the water, through the clothing with which the instrument is covered, is observed and noted down.

Now, as the time of cooling through any given interval of the scale of the thermometer, (or from any given degree above the temperature of the air of the room, to any other given lower degree, but still above the temperature of the air of the room,) will be longer, or shorter, as the covering of the instrument is more or less adapted for confining heat, it is evident, that the relative warmth of clothing of different kinds, may be very accurately determined by experiments of this sort.

I provided four instruments of this kind, all very nearly of the same dimensions. Their cylindrical bodies are each 4 inches in diameter, and 4 inches long; and their cylindrical necks are about $\frac{8}{10}$ of an inch in diameter, and 4 inches in length. This neck is placed in the centre of the circular flat top, or upper end, of the vertical cylindrical body; and, opposite to it, in the centre of the flat bottom of the body, there is a hollow cylinder, $\frac{8}{10}$ of an inch in diameter, and 3 inches long, projecting downwards, into which a vertical cylinder of wood is fitted, on the top of which the instrument is supported, in such a manner that the air has free access to every part of it. This cylinder of wood constitutes a part of the wooden stand above-mentioned.

As the thermometer is placed in the axis of the cylindrical vessel, and as its bulb is just as long as the body of this vessel, it is evident that it must ever indicate the *mean temperature* of

the water in the vessel, however different the temperature of that water may be at different depths.

The thermometer is firmly supported in its place, by causing a part of the lower end of its scale to enter the neck of the cylindrical vessel, and to fit it with some degree of accuracy, but not so nicely as to be in danger of sticking fast in it.

The lower end of the bulb of the thermometer does not absolutely touch the bottom of the vessel, but it is very near touching it.

Figure 1 (Plate IV.) will give a clear idea of this instrument, placed on its wooden stand, which is so contrived, that the instrument may be placed higher, or lower, at pleasure.

The foregoing description of this instrument is so particular, that the figure will be easily understood, without any farther illustration. The cylindrical vessel is represented placed on the stand, with its thermometer in its place.

As, in some of the first experiments I made with this instrument, I found it difficult to apply the coverings which I used, to the ends of the body of the instrument, I endeavoured, by covering up those ends with a permanent and very warm covering, to oblige most of the heat to pass off through the vertical sides of the instrument; to which it was easy to fit almost any kind of covering, and more especially coverings of various thicknesses of confined air, the relative warmth of which I was very desirous of ascertaining.

The means I employed for covering up the ends of the instrument were as follows. Having provided two thin cylindrical wooden boxes, (like common pill-boxes, but much larger,) something less in diameter than the body of the instrument, and $2\frac{1}{2}$ inches deep, I dried them as much as possible; and, after

having varnished them, within and without, with spirit varnish, I covered them, within and without, with fine wove writing-paper, and then gave the paper three coats of the same varnish. I then perforated the bottoms of these boxes with round holes, just large enough to admit the neck of the instrument, and the cylindrical projection at its bottom; and then inverted them over the two ends of the instrument, filling the boxes at the same time with *eider-down*.

These boxes were fixed and confined in their places, by means easy to be imagined; and, in order to confine the heat still more effectually, each of the boxes was covered on the outside with a cap of fur, as often as the instrument was used; as was also that part of the neck of the instrument which projected above the box.

Two of the instruments, which I shall distinguish by the numbers 1 and 2, were covered up at their ends in this manner: the other two instruments, No. 3 and No. 4, were left in the state represented by the Figure 1; that is to say, the ends of their cylindrical bodies were not covered with permanent coverings.

In each experiment, two similar instruments (No. 1 and No. 2, for instance, or No. 3 and No. 4) were used, the one *naked*, and the other *covered;* and, as the naked instrument always served as a standard, with which the results of the experiments made with the other were compared, it is evident that this arrangement rendered the general results of the experiments much more satisfactory and conclusive than they could possibly have been, had the experiments made on different days, and with various kinds of covering, been made singly, or unaccompanied by a fixed and invariable standard.

The experiments were made, and registered, in the following

manner: the two instruments used in the experiment, placed on their wooden stands, being set down on the floor, were filled to within about $1\frac{1}{2}$ inch of the tops of their cylindrical necks with boiling hot water; and, a thermometer being put into each of them, they were placed, at the distance of 3 feet from each other, on a large table, in a corner of a large quiet room,* where they were suffered to cool, undisturbed. Near them, on the same table, and at the same height above the table, there was placed another thermometer, (suspended in the air, to the arm of a stand,) by which the temperature of the air of the room was ascertained from time to time.

No person was permitted to pass through the room, while an experiment was going on; and, in order to prevent, as far as it was possible, all those currents of air in the room which were occasioned by partial heat, produced by the light which came in at the windows, the window-shutters were kept constantly shut; one of them only being opened for a moment, now and then, just to observe the thermometers, and note down the progress of the experiment.

The results of each experiment were entered on a separate sheet of paper; which paper was previously prepared for that use, by being divided into separate vertical columns, by lines drawn with a pen, and ruled in parallel horizontal lines with a lead pencil.

The following is an exact copy of one of these register-sheets; and contains the results of an actual and very interesting experiment, which lasted 26 hours.

* This room, which is adjoining to my laboratory, in my house at Munich, is 19 feet wide, 24 feet long, and 13 feet high.

"Experiments on Heat, made at Munich, 11th March, 1803.
"The large cylindrical Vessels, No. 1 and No. 2, (made of thin "sheet brass,) were filled with hot Water, and exposed to "cool in the Air of a large quiet Room. The Ends of both "these Instruments were well covered with warm Clothing, "Furs, *&c.* The vertical polished Sides of No. 1 were *naked.* "The Sides of No. 2 were *covered* with one Thickness of fine "white *Irish Linen*, which had been worn, strained over the "metallic Surface."

Time.		Temperature		Temperature of the air.
h.	min.	of No. 1, *naked.*	of No. 2, *covered.*	
10	10	$126\frac{1}{2}$°	126°	$43\frac{1}{4}$°
—	30	$109\frac{1}{2}$	$106\frac{1}{2}$	$43\frac{1}{2}$
—	45	105	$100\frac{1}{8}$	$43\frac{3}{4}$
11	—	$101\frac{1}{4}$	$94\frac{3}{4}$	44
—	$2\frac{1}{2}$	—	94	—
—	15	$97\frac{1}{2}$	$90\frac{1}{4}$	—
—	30	94	$86\frac{1}{4}$	—
—	39	—	84	—
—	45	$91\frac{1}{4}$	$82\frac{1}{2}$	—
12	—	$88\frac{1}{2}$	$79\frac{3}{8}$	—
—	15	$85\frac{1}{2}$	76	—
—	25	84	—	—
—	30	—	$74\frac{1}{2}$	—
—	45	80	70	—
1	—	78	$68\frac{1}{8}$	—
—	30	$74\frac{1}{4}$	$64\frac{1}{4}$	—
2	—	$71\frac{1}{8}$	$61\frac{1}{2}$	$43\frac{3}{4}$
—	30	$68\frac{1}{8}$	$58\frac{3}{4}$	$43\frac{1}{2}$
3	—	$65\frac{3}{4}$	$56\frac{3}{4}$	—
—	30	$63\frac{1}{2}$	$54\frac{3}{4}$	—

Time.		Temperature		Temperature of the air.
h.	min.	of No. 1, *naked.*	of No. 2, *covered.*	
4	—	$61\frac{3}{4}$°	$53\frac{1}{2}$°	$43\frac{1}{2}$°
—	30	$59\frac{1}{2}$	52	—
5	30	57	$49\frac{3}{4}$	$42\frac{1}{2}$
6	—	$55\frac{1}{2}$	$49\frac{1}{8}$	—
—	30	$54\frac{1}{4}$	$48\frac{1}{4}$	—
7	—	$53\frac{1}{2}$	$47\frac{1}{2}$	42
8	—	$51\frac{1}{2}$	$46\frac{1}{2}$	—
9	—	50	$45\frac{3}{4}$	—
10	—	49	45	—
8	12th Mar.	43	42	40
The instruments were now removed into a warm-room.				
8	2	43	42	62
—	32	$44\frac{3}{4}$	$44\frac{3}{4}$	$62\frac{1}{2}$
—	47	46	$46\frac{1}{2}$	63
9	24	48	$49\frac{1}{2}$	—
10	—	50	52	—
—	41	$51\frac{1}{2}$	$53\frac{7}{8}$	—
12	—	54	$56\frac{1}{2}$	—
12	26	$54\frac{1}{2}$	57	—
An end was now put to the experiment.				

Though it was easy to discover, by a single glance at the register, whether a covering which was put over one of the instruments prolonged the time of its cooling or not; yet, in order to compare the results of different experiments, and particularly of such as were made on different days, so as to determine with precision *how much* warmer one kind of covering was than another, it was necessary to fix on some particular interval in the scale of the thermometer, or number of degrees, commencing at some certain invariable number of degrees above the temperature of the air by which the instrument was surrounded, in order that the warmth of the covering, or its power of confining heat, might with certainty be estimated by the time employed in cooling through that interval.

By the results of a great number of experiments I found, that the same instrument cooled through any given (small) number of degrees, (10 degrees, for instance,) in very nearly the same time, whatever was the temperature of the air of the room; provided always, that the point from which these 10 degrees commenced, was at the same given number of degrees above the temperature of the air at the time being.

The interval I chose for comparing the results of my experiments, is that which commences with the *fiftieth*, and ends with the *fortieth* degree of Fahrenheit's thermometer, *above the temperature of the air in which the instrument is exposed to cool.* When, for instance, the air was at 58°, the interval commenced at the 108th degree, and ended at the 98th. When the air was at $64\frac{1}{2}$°, it commenced at $114\frac{1}{2}$°, and ended at $104\frac{1}{2}$°.

That the same instrument, exposed to cool in the air, does in fact cool the same number of degrees in the same time, very nearly, when the given interval of the scale of the thermometer

is reckoned from the same height, or given number of degrees above the temperature of the air at the time when the experiment is made, will appear from the following results of 11 different experiments, made on different days, and when the air in which the instrument was exposed to cool was at different degrees of temperature.

The large cylindrical vessel, No. 1, having its two ends well covered up with eider-down, furs, &c. its vertical sides being exposed *naked* to the air, in a large quiet room, was found to cool 10 degrees, *viz.* from the 50th to the 40th degree above the temperature of the air in which it was exposed, as follows.

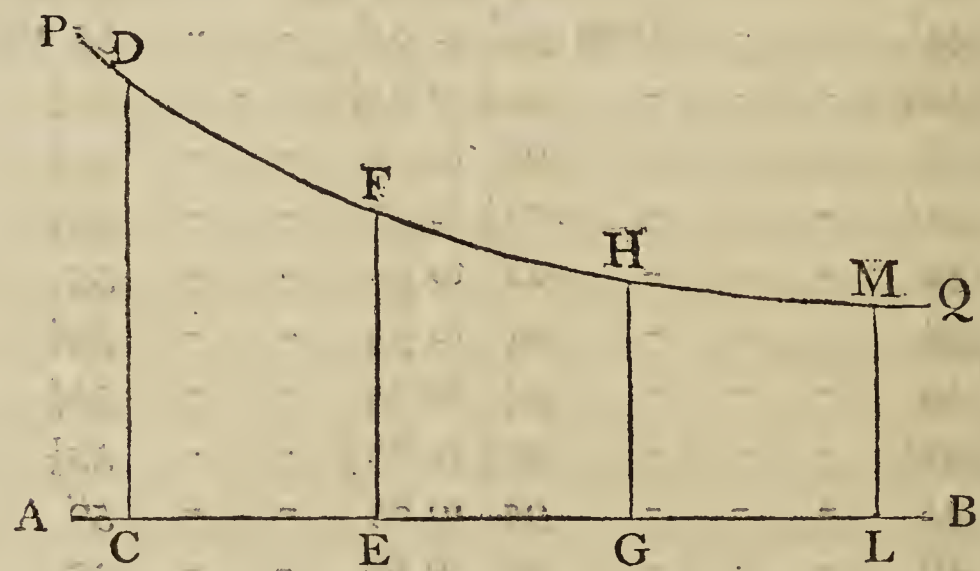
Temperature of the air.	Degrees cooled.	Time employed in cooling.
44°	from 94° to 84°	55 minutes.
$45\frac{1}{4}$	$95\frac{1}{4}$ to $85\frac{1}{4}$	$55\frac{1}{2}$
48	98 to 88	$55\frac{1}{4}$
$51\frac{1}{2}$	$101\frac{1}{2}$ to $91\frac{1}{2}$	$55\frac{1}{2}$
52	102 to 92	55
54	104 to 94	$54\frac{1}{4}$
44	94 to 84	$55\frac{5}{6}$
$42\frac{1}{2}$	$92\frac{1}{2}$ to $82\frac{1}{2}$	$55\frac{1}{3}$
45	95 to 85	56
46	96 to 86	55
44	94 to 84	$55\frac{1}{3}$

The fact which these experiments are here brought to prove, has likewise been confirmed by other experiments, made with other instruments, and at times when the temperature of the air has been as high as 64°; but I will not take up the time of the Society, by giving a particular account of them in this place.

As it sometimes happened, though very seldom, in the course

of an experiment, (which commonly lasted several hours,) that I was called away, and was not present to observe the thermometer, at the moment of the passage of the mercury through one or both of those points of its scale which formed the limits of the given interval, chosen as the standard for a comparison of the results of the experiments with each other, it became a matter of considerable importance, to find means for supplying these accidental defects, and ascertaining the points in question by interpolation.

In order to facilitate the means of doing this, I endeavoured to investigate the law of the cooling of hot bodies in a cold fluid medium; and I found reason to conclude,



That if, on the right line AB, a perpendicular CD be taken, equal to the difference of the temperatures of the hot body and of the colder medium, expressed in degrees of the thermometer; and, after a certain given time, represented by CE, taken on the line AB, at the point E, another perpendicular EF be erected, and EF be taken equal to the difference of the temperatures after the time represented by CE has elapsed; and if the

perpendiculars GH and LM be drawn, representing the difference of the temperatures after the times EG and GL have elapsed, a curved line PQ, drawn through the points D, F, H, M, will be the logarithmic curve; or, if it vary from that curve, its variation, within the limits answering to a change of temperature amounting to a few degrees, (especially if they be taken when the temperature of the hot body is about 40 or 50 degrees above that of the medium,) will be so very small, that no sensible error will result from a supposition that it is the logarithmic curve, in supplying, by computation, any intermediate observations, which happen to have been neglected in making an experiment.

These computations are very easily made, with the assistance of a table of logarithms, in the following manner.

Supposing CD, CG, and GH, to have been determined by actual observation; and that it were required to ascertain, by computation, the absciss CE, corresponding to any given intermediate ordinate EF, or, (which is the same thing,) to determine at *what time* the cooling body was at any given intermediate temperature (= EF) between that (= CD) which it was found by observation to have at the point C, and that (= GH) which it was found to have after the time represented by the line CG had elapsed;

It is log. CD — log. GH, is to CG as 1 to m; (= modulus = the subtangent of the curve at the point D.)* And CE = m × log. CD — log. EF.

* The subtangent shows in what time the instrument would cool down to the temperature of the air in which it is placed, were its velocity of cooling at the point D to be continued *uniformly* from that point; and, as the subtangent of the logarithmic curve is *constant*, if PQ were the logarithmic curve, it would follow, that the velocity

If, for instance, in the experiment of the 11th March, (the details of which have just been given,) the time when the instrument No. 2, in cooling, passed the important point of 94°, had not been observed, this neglect might have been supplied, by computation, in the following manner.

It is CD $= 94\frac{3}{4}°$, the nearest *observed* temperature higher than EF ($= 94°$) and GH $= 90\frac{1}{4}°$, the nearest observed temperature below that of 94°; and CG $= 15$ minutes, or 900 seconds, $=$ the time elapsed between the two observations.

$$\text{It is log. } 94\tfrac{3}{4} = 1.9765792$$
$$\text{And log. } 90\tfrac{1}{4} = \underline{1.9554472}$$
$$\text{Log. CD} - \text{log. GH} = 0.0211320$$

And 0.0211320 is to 900 ($=$ CG) as 1 to 42590 $= m$.

$$\text{And again, log. } 94\tfrac{3}{4} = 1.9765792$$
$$\text{Log. } 94 = \underline{1.9731279}$$
$$\text{Log. CD} - \text{log. EF} = 0.0034513$$

42590×0.0034513 ($= m \times$ log. CD — log. EF) $= 147$ seconds, $=$ 2 minutes and 27 seconds; which differs very little from $2\frac{1}{2}$ minutes, the observed time.

If, from the temperature observed at 11^h 30 min. $= 86\frac{1}{4}°$, and the temperature observed at 11^h 45 min. $= 82\frac{1}{2}°$, and the time which elapsed between these two observations, ($=$ 15 minutes) we were to determine, by computation, the time when the instrument was at the temperature of 84°, (the lower point of the standard interval of 10 degrees answering to the temperature of

with which a hot body cools in a fluid medium, is every where such, that were *that velocity* to be continued uniformly, the body would be cooled down to the temperature of the medium, *in the same time,* whatever might be the excess of the temperature of the hot body above that of the medium, at the moment when its velocity of cooling became uniform.

the air, $=44^\circ$, in which the instrument was cooled,) it will turn out, 8 minutes and 55 seconds after 11^h 30 min. The observed time was 11^h 39 minutes; which differs from the computed time no more than 5 seconds.

If it were strictly true, as a very great philosopher and mathematician has advanced, that the velocity with which a hot body exposed to cool in a cold fluid medium parts with its heat, is as the difference of the temperatures of the body and of the medium, it is most certain, that the curve PQ could be no other than the logarithmic curve. Perhaps it may be so in fact, and that the variations from it which my experiments indicated, were owing solely to the imperfection of the divisions of our thermometers. If it be so, it is not impossible to divide the scale of a thermometer in such a manner as to indicate with certainty *equal increments of heat*, as thermometers ought to do; but this is not the proper place to enlarge on this subject. I may perhaps return to it hereafter.

Passing over in silence, a number of experiments I made in order to get thoroughly acquainted with my new instruments, and to assure myself that the results of similar experiments made with them were uniform, and might be depended on, I shall now proceed to give an account of several experiments made with pointed views, the results of some of which were very interesting.

Experiment No. 1. The large cylindrical vessel No. 1, with its ends covered with warm clothing, in the manner before described, and its vertical sides (which were polished, and very clean and bright) exposed naked to the air, was filled with water nearly boiling hot, and placed on its wooden stand, on a table, in a

large quiet room, to cool; the air of the room being at the temperature of 45° FAHRENHEIT.

Another cylindrical vessel, No. 2, in all respects like No. 1, and with its ends covered in the same manner, but with its vertical sides covered with a single covering of fine Irish linen, (such as is sold in London for about 4*s.* *per* yard,) closely applied to the body of the instrument, was filled with hot water at the same time, and placed on the same table to cool.

This experiment lasted many hours; and, in that period, the temperature of the water, in each of the instruments, was carefully observed, and noted down, a great number of times.

The result of this experiment (the details of which have already been given) was very remarkable.

While the instrument No. 1, whose sides were *naked,* employed 55 minutes in cooling from the point of 94° to that of 84°, the instrument No. 2, whose sides were *covered with linen,* cooled through the same interval in $36\frac{1}{2}$ minutes.

Hence it appears, that clothing may, in some cases, expedite the passage of heat out of a hot body, instead of confining it in it.

Desirous of seeing whether the same covering would, or would not, expedite the passage of heat *into* the instrument; after having suffered both instruments to cool down to the temperature of about 42°, I removed them into a warm room, in which the air was at the temperature of 62°; and I found that the instrument No. 2, which was clothed, acquired heat considerably faster than the other, No. 1, which was naked.*

* The details of this experiment (which was made on the 11th of March, 1803) may be seen in page 83.

The discovery of these extraordinary facts surprised me, and excited all my curiosity; and I immediately set about investigating their cause.

As it is well known that air adheres with considerable obstinacy to the surfaces of some solid bodies, I conceived it to be possible, that the particles of air in immediate contact with the surface of the cylindrical vessel No. 1, might in fact be so attached to the metal as to adhere to it with some considerable force; and, if that were the case, as confined air is known to constitute a very warm covering, it appeared to me to be possible, that the cooling of the vessel No. 1, might have been retarded by such an invisible covering of confined air; which covering, in the experiment with the vessel No. 2, had been displaced, and in a great measure driven away, by the colder covering, of linen, by which the body of the instrument was closely embraced.

I conceived that the linen must have accelerated the cooling of the instrument, either by facilitating the approach of a succession of fresh particles of cold air, or by increasing the effects of *radiation*; and, with a view to elucidate that important point, the following experiments were made.

Exper. No. 2. Removing the linen with which the instrument No. 2 was clothed, I now covered the sides of that instrument with a thin transparent coating of glue; and, when it was quite dry and hard, I again filled the two instruments (No. 1 and No. 2) with hot water, and observed the times of their cooling as before.

Result, or time of cooling 10 degrees, reckoned from the 50th to the 40th degree above the temperature of the air in which the instruments were exposed to cool.

Instrument No. 1, sides *naked* - - -	55 min.
Instrument No. 2, sides *covered with one coating of glue*	$43\frac{1}{4}$ min.

N 2

When we consider this experiment with attention, we shall find reason to conclude, that if it were by facilitating the approach and temporary contact of a succession of fresh particles of the cold air of the room to the surface of the glue, (which was now in fact become the surface of the hot body,) that the cooling of the instrument was accelerated, the metal being as completely covered, and the air, supposed to be attached and fixed to its surface, as completely excluded by one coating of the glue as it could be by two, or more, two coatings could not possibly accelerate the cooling of the instrument more than one; but if, on the other hand, the cooling of the instrument in this experiment was accelerated, not by facilitating and accelerating the motions of the circumambient cold air, but by facilitating and increasing those *radiations* which are known to proceed from hot bodies, I conceived that two coatings of the glue might possibly accelerate the cooling of the vessel more than one. In order to put this conjecture to the test, I made the following decisive experiment.

Exper. No. 3. I now gave the instrument No. 2 a second coating of glue; and, when it was thoroughly dry, I repeated the experiment last mentioned, with the above variation; when I found the results to be as follows.

	Time of cooling the 10 degrees in question.
The instrument No. 1, *naked* metal - -	$55\frac{1}{3}$ min.
No. 2, *covered* with two coatings of glue	$37\frac{5}{6}$ min.

Finding that two transparent coatings of glue facilitated the cooling of this instrument even more than one coating, I washed off all the glue with warm water; then, making the instrument as clean and bright as possible, I covered its sides with

a coating of very fine, transparent, and colourless spirit varnish; and, after this coating of varnish had become quite dry and hard, I repeated the experiment above-mentioned; and, finding that this covering, like that of glue, expedited the cooling of the instrument, I first added a *second* coating of the varnish, and repeated the experiment again, and then added two coatings more, making *four* in all. Finding that the cooling of the instrument was more and more rapid, as the thickness of the varnish was increased, I now added four coatings more, making *eight* coatings in the whole, giving time for each new coating to dry thoroughly, before the next was applied; but I found, on repeating the experiment with this thick covering of varnish, that I had passed the limit of thickness which produced the greatest effect.

In order that the results of these experiments, with coatings of different thicknesses of spirit varnish, may be seen at one view, I shall here place them all together; and I shall place by the side of each, the result of the standard experiment, which was made at the same time, with the instrument No. 1, the sides of which were *naked.*

		Time employed in cooling through the given interval of 10 degrees.	
		Instrument No. 1, *varnished.*	Instrument No. 2, *naked.*
Exper. No. 4.	1 coating of varnish -	42 min. -	$55\frac{1}{2}$ min.
Exper. No. 5.	2 coatings - - -	$35\frac{3}{4}$ - -	$55\frac{1}{4}$
Exper. No. 6.	4 coatings - - -	$30\frac{1}{4}$ - -	$55\frac{1}{2}$
Exper. No. 7.	8 coatings - - -	$34\frac{1}{4}$ - -	55

Exper. No. 8. Desirous of finding out what effect *colour* would produce, I now painted the sides of the instrument No. 2 *black,* with lamp-black mixed up with size, (this paint being laid

upon the eighth coating of the varnish,) and, repeating the experiment, its results were as follows.

	Time employed in cooling through the given interval.
The instrument No. 1, *naked* - - -	$55\frac{1}{4}$ min.
Instrument No. 2, covered with 8 coatings of varnish, and painted *black*	34 min.

Exper. No. 9. Finding that the painting of this thick coating of varnish *black,* rendered the covering still colder, or accelerated the cooling of the instrument, I now washed off the black paint, with warm water; then, washing off all the varnish with hot spirit of wine, I painted the metallic sides of the instrument of a black colour, with lamp-black and size; and, when the paint was quite dry, I repeated the experiment so often mentioned; when the results were as follows.

	Time employed in cooling through the given interval.
The instrument No. 1, sides *naked* - -	$55\frac{1}{6}$ min.
No. 2, *painted black* - -	35 min.

Exper. No. 10. In order to find out whether the *black* colour had any particular efficacy in expediting the cooling of the instrument, or whether another colouring substance would not produce the same effect, when mixed up with the same size, I now washed off the black paint, and painted the sides of the instrument *white,* with whiting mixed up with size; and, on repeating the experiment, the results were as follows.

	Time of cooling through the given interval.
The instrument No. 1, *naked* - - -	$55\frac{1}{3}$ min.
No. 2, *painted white* - -	36 min.

As, in both the two last experiments, it was found necessary

to paint the body of the instrument three or four times over, in order to cover the polished metal so completely as to prevent its shining through the paint; this of course occasioned the surface of the metal to be covered with a thick coating of size, which, no doubt, affected very sensibly the results of the experiment, and rendered it impossible to determine, in a satisfactory manner, what the effects really were, which were produced by the *different colours* used in the two experiments.

Exper. No. 11. With a view to throw some more light on this interesting subject, having washed off the paint from the instrument No. 2, I now rendered its sides of a perfectly deep black colour, by holding it over the flame of a wax candle; and, repeating the usual experiment, the results were as follows.

	Time of cooling through the standard interval.
The instrument No. 1, *naked* - - -	$55\frac{7}{8}$ min.
No. 2, *blackened* - - -	$36\frac{1}{8}$ min.

In order to ascertain the quantity of matter which composed this black covering, I weighed a small piece of clean and very fine linen; and, having wiped off with it all the black matter from the body of the instrument No. 2, in such a manner that the whole of it remained attached to the linen, I weighed it again, and by that means discovered that the whole of this black substance, which had so completely covered the sides of the instrument (a surface of polished brass = 50 superficial inches) that the metal did not shine through it in any part, weighed no more than $\frac{1}{18}$ of a grain Troy.

How this very thin covering, which, if the specific gravity of the black matter were only equal to that of water, would amount to no more than $\frac{1}{4509}$ of an inch in thickness, could expedite

the cooling of the instrument, in the manner it was found to do, is what still remains to be shown: but, before I proceed any farther in these abstruse enquiries, I shall make a few observations relative to the results of the foregoing experiments.

Although we may with safety presume, that the velocities with which the heat escaped *through the sides of the instruments,** were nearly as the times inversely taken up in cooling through the given interval of 10 degrees; yet, as some heat must have made its way, in the course of the experiment, *through the ends of the instrument,* notwithstanding all the care that was taken to prevent it, by covering them up with warm clothing, it is necessary, in order to be able to compare the results of the preceding experiments in a satisfactory manner, to find out how much of the heat made its escape through the covered ends of the instruments, during the time the instruments were cooling through the interval in question.

In order to determine that point, I now removed the covering from the ends of the instrument No. 1; and, when it was quite naked, I found, on making the experiment, that it cooled through the given interval in $45\frac{1}{2}$ minutes.

When its two ends and its cylindrical neck were covered up

* I have found myself obliged in this, as in many other places, to make use of language which is far from being as correct as I could wish. I do not believe that heat ever *makes its escape* in the manner here indicated; but I could not venture to use uncommon expressions, in pointing out the phenomena in question, however well adapted such expressions might be to describe the events which really take place. If it should be found that *caloric,* like *phlogiston,* is merely a creature of the imagination, and has no real existence, (which has ever appeared to me to be extremely probable,) in that case, it must be incorrect to speak of heat as *making its escape* out of one body, and *passing* into another: but how often are we obliged to use incorrect and figurative language, in speaking of natural phenomena!

with warm clothing, I found, by taking the mean of the results of several experiments, that it required $55\frac{1}{2}$ minutes to cool through the same interval.

On measuring the instrument with care, I found its dimensions to be as follows.

		Inches.
Diameter of the body of the instrument	-	= 4.03
Length of the body	- - - - -	= 3.96
Diameter of the neck of the instrument	-	= 0.8
Length of the neck	- - - - -	= 4.

The superficies of the different parts of the instrument are therefore as follows.

Superficies of the vertical sides of the body $(= 4.03 \times 3.14159 \times 3.96) = 50.136$ inches.

Superficies of the flat circular bottom of the instrument, $\left(= 4.03 \times 3.14159 \times \frac{4.03}{4}\right) = 12.755$ inches; deducting nothing for that part which is covered by the end of the tube, which serves as a support for the instrument.

Superficies of the flat circular top of the instrument, (after deducting 0.502 of a superficial inch, for the circular hole in its centre, made to receive the lower end of the cylindrical neck,) $= 12.253$ inches.

Superficies of the cylindrical neck of the instrument $(= 0.8 \times 3.14159 \times 4) = 10.051$ inches.

Supposing now, that the heat passes with equal velocity through the surface of all the different parts of the instrument, when the instrument is naked, we can determine the quantity of heat which escaped through the ends and neck of the instrument, in the experiments in which those parts of the instrument were covered with warm clothing.

The whole of the metallic surface exposed to the air, in the experiments made with the instrument when it was quite naked, amounted to 85.195 superficial inches; namely;

Surface of its vertical sides	- -	= 50.136 inches.
of its lower end	- -	= 12.755
of its upper end	- -	= 12.253
of its neck	- - -	= 10.051
	Total surface	= 85.195 inches.

When the instrument was exposed quite naked to the air, it was found to cool through the standard interval of 10 degrees, in $45\frac{1}{2}$ minutes.

Assuming now any given number, as the measure of the whole quantity of heat given off by the instrument during the period above-mentioned, we can ascertain what part or proportion of that quantity passed off through the sides of the instrument; and what part of it must have made its escape through its ends, and through the sides of its neck.

As the quantities of heat given off are supposed to have been as the quantities of surface exposed to the air, if we suppose the whole quantity of heat lost by the instrument to be = 10000 parts, the quantity which passed through the vertical sides of the instrument in $45\frac{1}{2}$ minutes, in the experiment, must have amounted to 5885 parts. For, the whole of the surface of the instrument, = 85.195 superficial inches, is to the whole of the heat given off, = 10000, as the surface of the vertical sides of the instrument, = 50.136 superficial inches, to the quantity of heat which must have passed off through that surface in the given time, = 5885.

Now, as we may with safety conclude, that the quantity of

heat which passes off through a *given surface* must be as the times elapsed, all other circumstances being the same, we can determine how much of the heat given off by the instrument, in those experiments in which its ends were covered, passed through the sides of the instrument; and, consequently, how much of it must have made its way through its ends and neck, notwithstanding their being covered.

The instrument with its ends and neck covered up with eider-down, furs, &c. was found to cool through the standard interval of 10 degrees in $55\frac{1}{2}$ minutes. Now, as only 5885 parts of heat were found to pass through the naked vertical sides of the instrument in $45\frac{1}{2}$ minutes, no more than 7015 parts could have passed through the same surface in $55\frac{1}{2}$ minutes; consequently, the remainder of the heat lost by the instrument, in the experiment in question, amounting to 2985 parts, must necessarily have made its way through the covered ends and neck of the instrument, in the given period, $55\frac{1}{2}$ minutes.

Taking it for granted that these computations are well founded, we may now proceed to a more exact determination of the relative quantities of heat which made their way through the sides of the instrument No. 2, when its sides were exposed naked to the air, and when they were covered with the different substances which appeared to facilitate the escape of the heat.

In the experiment No. 11, when the sides of the instrument were made quite black, by holding it over the flame of a wax candle, the instrument cooled through the standard interval of 10 degrees in $36\frac{1}{8}$ minutes.

In that time, a quantity of heat = 1942 parts, must have passed off through the covered ends and neck of the instrument; for, if a quantity = 2985 parts could pass off that way in $55\frac{1}{2}$

minutes, the quantity above-mentioned (= 1942 parts) must have escaped in $36\frac{1}{8}$ minutes.

This quantity, = 1942 parts, taken from the whole quantity, = 10000 parts, lost by the instrument in cooling through the interval in question, leaves 8058 parts, for the quantity which made its escape through the sides of the instrument, in the experiment in question.

Now, if a quantity of heat = 7015 parts, requires $55\frac{1}{2}$ minutes to make its way through the naked sides of the instrument, (as we have just seen,) it would require $63\frac{3}{4}$ minutes, for the quantity in question, = 8058 parts, to pass off through the same surface.

But, when that surface was blackened over the flame of a candle, that quantity of heat passed off through it in $36\frac{1}{8}$ minutes.

Hence it appears, that the velocity with which heat is given off from the naked surface of a heated metal exposed to cool in the air, is to the velocity with which it is given off by the same metal when its surface is blackened in the manner above described, as $36\frac{1}{8}$ to $63\frac{3}{4}$, or as 5654 to 10000, very nearly; for the velocities are as the times of cooling, inversely.

Again, in the experiment No. 6, the sides of the instrument No. 2 being covered with four coatings of spirit varnish, the instrument was found to cool through the given interval of 10 degrees in $30\frac{1}{4}$ minutes,

In that time, a quantity of heat = 1627 parts, must have made its way through the covered ends of the instrument; and the remainder, = 8373 parts, must have made its way through its varnished sides.

This quantity, = 8373 parts, would have required $66\frac{1}{4}$ minutes, to have made its way through the naked sides of the instrument; and, as it actually made its way through the varnished sides of

the instrument in $30\frac{1}{4}$ minutes, it appears that the velocity with which the heat was given off from the naked metallic surface, was to the velocity with which it was given off from the same surface covered with four coatings of spirit varnish, as $66\frac{1}{4}$ to $30\frac{1}{4}$, or as 10000 to 4566.

Without pursuing these computations any farther, at present, and without stopping to make any remarks on the curious facts they present to us, I shall hasten to experiments from the results of which we shall obtain more satisfactory information. But, before I proceed any farther, I must give an account of an instrument I contrived for measuring, or rather for *discovering*, those very small changes of temperature in bodies, which are occasioned by the radiations of other neighbouring bodies, which happen to be at a higher, or at a lower temperature.

This instrument, which I shall take the liberty to call a *thermoscope*, is very simple in its construction. Like the hygrometer of Mr. Leslie, (as he has chosen to call his instrument,) it is composed of two glass balls, attached to the two ends of a bent glass tube; but the balls, instead of being near together, are placed at a considerable distance from each other; and the tube which connects them, instead of being bent in its middle, and its two extremities turned upwards, is quite straight in the middle, and its two extremities, to which its two balls are attached, are turned perpendicularly upwards, so as to form each a right angle with the middle part of the tube, which remains in a horizontal position.

At one of the elbows of this tube, there is inserted a short tube, of nearly the same diameter, by means of which, a very small quantity of spirit of wine, tinged of a red colour, is introduced into the instrument; and, after this is done, the end of

this short tube (which is only about an inch long) is sealed hermetically; and all communication is cut off, between the air in the balls of the instrument and in its tube, and the external air of the atmosphere.

A small *bubble* of the spirit of wine (if I may be allowed to use that expression) is now made to pass out of the short tube, into the long connecting tube; and the operation is so managed, that this bubble (which is about $\frac{1}{4}$ of an inch in length) remains stationary, at or near the middle of the horizontal part of the tube, *when the temperature (and consequently the elasticity) of the air in the two balls, at the two extremities of the tube, is precisely the same.*

By means of a scale of equal parts, attached to the horizontal part of the connecting tube, the position of the bubble can be ascertained, and its movements observed.

If now, the bubble being at rest in its proper place, one of the balls of the instrument be exposed to the calorific rays which proceed in all directions from a hot body, while the other ball is defended from those rays by a screen, the air in the ball so exposed to the action of these rays, will be heated; and, its elasticity being increased by this additional heat, its pressure will no longer be counterbalanced by the elasticity of the colder air in the other ball, and the bubble will be forced to move out of its place, and to take its station nearer to the colder ball.

By presenting two hot bodies, at the same time, to the two balls of the instrument, taking care that each ball shall be defended from the action of the hot body presented to the opposite ball, the distances of these hot bodies from their respective balls may be so regulated, that their actions on those balls may be equal, however the temperatures of those hot bodies may differ,

or however different may be the quantities, or intensities, of the calorific rays which they emit.

The instrument will show, with the greatest certainty, when the actions of these hot bodies on their respective balls are equal; for, until they become *unequal,* the bubble will remain immoveable in its place.

And, when the actions of two hot bodies on the instrument are equal, the relative intensities of the rays they emit may be ascertained, by the distances of the bodies from the balls of the instrument.

If their distances from their respective balls are equal, the intensities of the rays they emit must of course be equal.

If those distances are unequal, the intensities will probably be as the squares of the distances, inversely.

A distinct and satisfactory idea may be formed, of the instrument I have been describing, from Figure 2.

AB is a board, 27 inches long, 9 inches wide, and 1 inch thick, which serves as a support for the bent tube CDE, at the two extremities of which the two balls are fixed. The two projecting ends of the tube, C and E, which are in a vertical position, are each 10 inches long; and the horizontal part D of the tube, which is fastened down on the board, is 17 inches in length.

The balls are each 1.625 inches in diameter. The diameter of the tube is such, that 1 inch of it in length would contain 15 grains Troy of mercury.

The pillar F, which, by means of a horizontal arm projecting from it, serves for supporting the circular vertical screen represented in the figure, is firmly fixed in the board AB.

This circular screen (which is made of pasteboard, covered

on both sides with gilt paper) serves for preventing one of the balls of the instrument from being affected by the calorific rays proceeding from a hot body which is presented to the opposite ball.

Besides the circular screen represented in the figure, several other screens are used in making experiments; for the instrument is so extremely sensible, that the naked hand presented to one of the balls, at the distance of several inches, puts the bubble in motion; and it is affected very sensibly by the rays which proceed from the person who approaches it to make the experiments, unless care be taken, by the interposition of screens, to prevent those rays from falling on the balls. These screens can be best and most readily made, by providing light wooden frames, about 2 feet square, and half an inch in thickness, and covering them on both sides, first with thick cartridge paper, and then with what is called gilt paper; the metallic substance (copper) with which one side of the paper is covered being on the outside.

To support a moveable screen of this kind in a vertical position, it must of course be provided with a foot or stand. Those I use, are fastened to one side of a pillar of wood, by two screws; one of which passes through the centre of the screen, where the cross bars belonging to the frame of the screen meet; and the other through the middle of the piece of wood which forms the bottom of the screen. This pillar of wood, which is turned in a lathe, is $12\frac{1}{2}$ inches high, and is firmly fixed, at its lower end, in a piece of wood, 8 inches square, and 1 inch thick, which serves as a stand or foot, for supporting it.

As, in making experiments with this *thermoscope*, it is frequently necessary to remove the hot bodies, that are presented

to it, farther from it, or to bring them nearer to it; in order that this may be done easily, and expeditiously, by one person, and without its being necessary for him to remove his eye from the bubble, (which he should constantly have in his view,) I make use of a simple machine, which I have found to be very useful.

It is a long and shallow wooden box, open at both ends. It is 6 feet long, 12 inches wide, and 5 inches deep, measured on the outside: its vertical sides are made of $1\frac{1}{2}$-inch deal; its bottom and top, of inch deal. A part only of the top or cover of this box is fixed down on the sides, and is immoveable. The part of the cover which is fixed, and on which the thermoscope is placed, occupies the middle of the box, and is 13 inches in length. On the right and left of this fixed part, the top of the box is covered by a sliding board, 2 feet 3 inches long, which passes in deep grooves, made to receive it, in the sides of the box. A rack is fixed to the under side of each of these sliding boards; and there is a small cog wheel in the box, the axis of which passes through the sides of the box, and is furnished with a winch in the front of the box. By turning round these wheels, by means of their winches, (both of which can be managed by the same person, at the same time,) the sliders may be moved backwards and forwards, at pleasure.

In order to ascertain with facility and dispatch, the distances of the hot bodies from their respective balls, the top of the front side of the wooden box is divided into inches, on each side of the fixed part of the cover of the box; and there is a *nonius* belonging to each of the sliders, which is placed in such a manner as to indicate, at all times, the exact distance of the hot body from its corresponding ball.

The level of the upper surface of that part of the cover

which is fixed, is about $\frac{1}{8}$ of an inch higher than the level of the upper surface of the sliders; in order that, when a thermoscope longer than this fixed part is placed on it, the sliders may pass freely under its two projecting ends, without deranging it.

It is evident, from this description, that, by placing the thermoscope on the fixed part of the cover of the box, with its two balls in a line parallel to the axis of the box, and by placing the two hot bodies presented to the two balls of the instrument (elevated to a proper height) on stands set down on the sliders, an observer, by taking the two winches in his hands, keeping his eye fixed on the bubble, may, with the greatest facility, so regulate the distances of the hot bodies from their respective balls, that the bubble shall remain immoveable in its place.

In order to be able to ascertain precisely the temperatures of the hot bodies presented to this instrument, and in order that their surfaces might be equal, two equal cylindrical vessels, of thin sheet brass, with oblique cylindrical necks, were provided, of the form represented in Figure 3.

This cylindrical vessel, which is placed in a horizontal position, in order that its flat bottom may be presented, *in a vertical position,* to one of the balls of the thermoscope, is so fixed to a wooden stand, of a peculiar construction, that it may be raised, or lowered, at pleasure. This is necessary, in order that its axis may be in the continuation of a line passing through the centres of the two balls of the thermoscope.

This cylindrical vessel is 3 inches in diameter, and 4 inches in length; and its oblique cylindrical neck is 0.86 of an inch in diameter, and 3.8 inches in length.

The neck of this vessel is inserted *obliquely* into its cylindrical

body, in order that the water with which it is occasionally filled may not run out of it, when the body of the vessel is laid down in a horizontal position, in the manner represented in the above-mentioned figure.

A thermometer, with a cylindrical bulb 4 inches in length, being inserted into the body of this vessel, through its neck, shows the temperature of the contained water.

Care is necessary, in constructing a thermoscope, to choose a tube of a proper diameter: if its bore be too small, it will be found very difficult to keep the spirit of wine in one mass; and, if it be too large, the little horizontal column it forms, (which I have called a bubble,) will be ill defined at its two ends, which will render it difficult to ascertain its precise situation. After a number of trials, I have found, that a tube, the bore of which is of such a size that 1 inch of it in length contains about 15 or 18 grains Troy of mercury, answers best. For a tube of that size, the balls may be about $1\frac{1}{2}$ inch in diameter; and they should both be painted black, with Indian ink, which renders the instrument more sensible.

I have an instrument of this kind, the tube of which is quite filled with spirit of wine, excepting only the space occupied by a small bubble of air, which is introduced into the middle of the horizontal part of the tube; but it does not answer so well as those which contain only a very small quantity of that liquid, sufficient to form a small bubble.

But, without enlarging any farther, at present, on the construction of these instruments, I now proceed to give an account of the experiments for which they were contrived.

Having found abundant reason to conclude, from the results of the experiments of which an account has already been given,

that all the heat which a hot body loses, when it is exposed in the air to cool, is not given off to the air which comes into contact with it; but that a large proportion of it escapes in rays, which do not heat the transparent air through which they pass, but, like light, generate heat only when, and where, they are stopped and absorbed; I suspected that, in every case when, in the foregoing experiments, the cooling of my instruments was expedited by coverings applied to their metallic surfaces, those coverings must, by some means or other, have facilitated and accelerated the emission of calorific rays from the hot surface.

Those suspicions implied, it is true, the supposition that different substances, heated to the same temperature, emit unequal quantities of calorific rays; but I saw no reason why this might not be the case in fact; and I hastened to make the following experiments, which put the matter beyond all doubt.

Exper. No. 12. Two equal cylindrical vessels, made of sheet brass, and polished very bright, each 3 inches in diameter, and 4 inches long, suspended by their oblique necks, in a horizontal position, (being placed on their wooden stands,) were filled with water at the temperature of 180°; and their circular flat bottoms were presented, in a vertical position, to the two balls of the thermoscope, at the distance of 2 inches.

When the two hot bodies were presented, at the same moment, to the two balls of the instrument, or, what was still better, when two screens were placed before the two balls, at the distance of about an inch, and, after the hot bodies were placed, these screens were both removed at the same instant, the small column of spirit of wine, which I have called a *bubble*, remained immoveable in its place, in the middle of the horizontal part of the tube of the instrument.

If one of the hot bodies was now brought nearer the ball to which it was presented, (the other hot body remaining in its place,) the bubble immediately began to move from the hot body which was advanced forward, towards the opposite ball, to which the other hot body was presented.

If, instead of advancing one of the hot bodies nearer the ball to which it was presented, it was drawn backward to a greater distance from it, the action of its calorific rays on the ball was diminished by this increase of distance; and, being overcome by the action of the rays from the hot body presented to the opposite ball, (at a smaller distance,) the bubble was forced out of its place, and obliged to move towards the ball which had been drawn backward.

When one of the hot bodies only was presented to one of the balls, the bubble was immediately put in motion; and, by bringing the hot body nearer to the ball, it might be driven quite out of the tube, into the opposite ball; this, however, should never be done, because it totally deranges the instrument, as it is easy to perceive it must do.

Having, by these trials, ascertained the sensibility and the accuracy of my instrument, I now proceeded to make the following decisive experiment.

Exper. No. 13. Having blackened the flat circular bottom of one of the cylindrical vessels, by holding it over the flame of a wax candle, I now filled both vessels again with water at the temperature of 180° F. and presented them, as before, to the two opposite balls of the instrument, at equal distances.

The bubble was instantly driven out of its place, by the superior action of the blackened surface; and did not return to its former station, till after the vessel which was blackened

had been removed to more than 8 inches from the ball to which it was presented; the other vessel, which had not been blackened, remaining in its former situation, at the distance of 2 inches from its ball.

The result of this experiment appeared to me to throw a new light on the subject which had so long engaged my attention; and to present a wide and very interesting field for farther investigation.

I could now account, in a manner somewhat more satisfactory, for those appearances in the foregoing experiments which were so difficult to explain,—for the acceleration of the passage of the heat out of my instruments, which resulted from covering them with linen, varnish, &c. and I immediately set about making a variety of new experiments, from which I conceived I should acquire a farther insight into those invisible mechanical operations which take place when bodies are heated and cooled.

Finding so great a difference in the quantities of calorific rays which are thrown off by the polished surface of a metal, when exposed *naked* to the cold air, and when *blackened*, I now proceeded to make experiments, to ascertain whether or not all those substances with which the sides of my cylindrical vessels had been covered, and which had been found to expedite the cooling of those instruments, would also facilitate the emission of calorific rays from the surfaces of the instruments I presented to the balls of my thermoscope; and I found this to be the case in fact.

As the results of all these experiments proved, in the most decisive manner, that all the substances which, when applied to the metallic surfaces of my large cylindrical vessels, had expedited their cooling, facilitated and expedited the emission of

calorific rays, I could no longer entertain any doubts respecting the agency of *radiation*, in the heating and cooling of bodies. Many important points however still remained to be investigated, before distinct and satisfactory ideas could be formed, respecting the nature of those rays, and the mode of their action.

I had hitherto made use of but one metal (brass) in my experiments; and that was not a simple, but a compound metal. The first subject of enquiry which presented itself, in the prosecution of these researches, was to find out whether or not similar experiments made with other metals would give similar results.

Exper. No. 14. Procuring from a gold-beater a quantity of leaf gold and leaf silver, about three times as thick as that which is commonly used by gilders, I covered the surfaces of the two large cylindrical vessels, No. 1 and No. 2, with a single coating of oil varnish; and, when it was sufficiently dry for my purpose, I gilt the instrument No. 1 with the gold leaf, and covered the other, No. 2, with silver leaf. When the varnish was perfectly dry and hard, I wiped the instruments with cotton, to remove the superfluous particles of the gold and silver, and then repeated the experiment so often mentioned, of filling the instruments with boiling hot water, and exposing them to cool, in the air of a large quiet room.

The time of cooling through the given interval of 10 degrees, was just the same as it was before, when the natural surface of these brass vessels was exposed *naked* to the air. I repeated the experiment several times, but could not find that the difference in the metals made any difference in the times of cooling.

Exper. No. 15. Not satisfied to rest the determination of so important a point on a trial with three metals only, brass, gold,

and silver, I now provided myself with two new instruments, the one made of lead, and the other covered with tinned sheet iron, improperly, in England, called tin.

As the *conducting power* of lead, with respect to heat, is much greater than that of any other metal, I conceived that, if the *radiation* of a body were any way connected with its *conducting power*, the cooling of the water contained in the leaden vessel, would necessarily be either more or less rapid than in a vessel constructed of any other metal.

The result of this experiment, as also the results of several others similar to it, showed that heat is given off with the same facility, or with the same celerity, from the surfaces of all the metals.

Is not this owing to their being all equally wanting in *transparency?* And does not this afford us a strong presumption that heat is, in all cases, excited and communicated by means of radiations, or *undulations*, as I should rather choose to call them?

I am sensible, however, that there is another and most important question to be decided, before these points can be determined; and that is, whether bodies are cooled in consequence of the rays they emit, or by those they receive?

The celebrated experiment of Professor PICTET, which has often been repeated, appears to me to have put the fact beyond all doubt, that rays, or emanations, which, like light, may be concentrated by concave mirrors, proceed from cold bodies; and that these rays, when so concentrated, are capable of affecting, in a manner perfectly sensible, a delicate air thermometer.

One of the objects I had principally in view, in contriving the

before described instrument, which I have called a thermoscope, was to investigate the nature and properties of those emanations; and to find out, if possible, whether they are not of the same nature as those calorific rays which have long been known to proceed from hot bodies.

My first attempts, in these investigations, were to ascertain the existence of those emanations universally; and to discover what visible effects they might be made to produce, independently of concentration by means of concave mirrors.

Exper. No. 16. My two horizontal cylindrical vessels, of sheet brass, (of the same form and dimensions,) having been made very clean and bright, were fixed to their stands; and, being elevated to a proper height to be presented to the balls of the thermoscope, were set down near that instrument, (which was placed on a table in a large quiet room,) where they were suffered to remain several hours, in order that the whole of this apparatus might acquire precisely the same temperature.

Day-light was excluded, by closing the window-shutters; and, in order that the thermoscope might not be deranged by the calorific rays proceeding from the person of the observer, on his entering the room to complete the intended experiments, screens were previously placed before the instrument, in such a manner that its balls were completely defended from those rays.

Things having been thus prepared, I entered the room as gently as possible, in order not to put the air of the room in motion, and, approaching the thermoscope, presented first one, and then the other cylindrical vessel, to one of the balls of the instrument; but it was not in the least degree affected by them,

the bubble of spirit of wine remaining immoveably in the same place.

Exper. No. 17. Having assured myself, by these previous trials, that the instrument was not sensibly affected by a bright metallic surface being presented to it, provided the temperature of the metal and that of the instrument were the same, I now withdrew one of the cylindrical vessels, and, taking it into another room, I filled it with pounded ice and water.

Entering the room again, I now presented the flat vertical bottom of this horizontal cylindrical vessel, filled with ice and water, to one of the balls of the thermoscope, at the distance of four inches.

The bubble of spirit of wine began instantly to move, with a slow regular motion, towards the cold body; and, having advanced in the tube about an inch, it remained stationary.

On bringing the cold body nearer the ball to which it was presented, the bubble was again put in motion, and advanced still farther towards the cold body.

Exper. No. 18. Although the result of the foregoing experiment appeared to me to afford the most indisputable proof of the *radiation* of cold bodies, and that the rays which proceed from them have a power of *generating cold* in warmer bodies which are exposed to their influence, yet, in a matter so extremely curious, and of such high importance to the science of heat, I was not willing to rest my enquiries on the result of a single experiment.

In order to vary the substance, or species of matter, presented cold to the instrument, and, at the same time, to remove all suspicion respecting the possibility of the effects observed being

produced by currents of cold air occasioned in the room by the presence of the cold body, I now repeated the experiment with the following variations.

The thermoscope was laid down on one side, so that the two ends of its tube, to which its balls were attached, instead of being vertical, were now in a horizontal position; and the cold body, instead of being presented to the ball of the instrument on one side of it, and on the same horizontal level with it, was now placed *directly under it*, and at the distance of 6 inches.

This cold body, instead of being a metallic substance, was a solid cake of ice, circular, flat, and about 3 inches thick, and 8 inches in diameter. It was placed in a shallow earthen dish, about 9 inches in diameter below, 12 inches in diameter above, at its brim, and 4 inches deep. The cake of ice being laid down on the bottom of the dish, the top of the dish was covered by a circular piece of thick paper, 14 inches in diameter, which had a circular hole in its centre, just 6 inches in diameter.

This earthen dish, containing the ice, and thus covered, was placed perpendicularly under one of the balls of the thermoscope, at such a distance that the centre of the upper surface of the flat cake of ice was 6 inches below the ball.

The result of this experiment was just what might have been expected: the ice was no sooner placed under the ball of the instrument, than the bubble of spirit of wine began to move towards that side where the cold body was placed; and it did not remain stationary, till after it had advanced more than an inch in the tube.

Exper. No. 19. Desirous of discovering whether the surface of a liquid emits frigorific or calorific rays, as solid bodies have been found to do, I now removed the cake of ice from the

earthen dish, and replaced it with an equal mass of ice-cold water.

The result of this experiment was, to all appearance, just the same as that of the last. The bubble moved towards the cold body, and took its station in the same place where it had remained stationary before. I found reason however to conclude, after meditating on the subject, that although the last experiment proves, in a most decisive manner, that radiations actually proceed from the surface of *water*, yet the proof of the radiation from the surface of ice, afforded by the preceding experiment, is not equally conclusive; for, as the temperature of the air of the room in which these experiments were made, was many degrees above the freezing point, it is possible, and even probable, that the surface of the ice was actually covered with a very thin, and consequently invisible, coating of water, during the whole of the time the experiment lasted.

Finding reason to conclude, that frigorific rays are always emitted by cold bodies, and that these emanations are very analogous to the calorific rays which hot bodies emit, I was impatient to discover, whether all cold bodies, at the same temperature, emit the same quantity of rays, or whether (as I had found to be the case with respect to the calorific rays emitted by hot bodies) some substances emit more of them, and some less.

With a view to the ascertaining of this important point, I made the following experiments.

Exper. No. 20. Having found that a metallic surface, rendered quite black by holding it over the flame of a wax candle, emits a much larger quantity of calorific rays, when hot, than the same metal, at the same temperature, throws off when naked, I was very curious to find out whether blackening the surface of

a cold metal, would or would not increase, in like manner, the quantity of frigorific rays emitted by it.

Having blackened, in the manner already described, the flat bottom, or rather end, of one of my horizontal cylindrical brass vessels with an oblique neck, I filled it with a mixture of ice and common salt; and, filling another vessel of the same kind, the bottom of which was not blackened, with the same cold mixture, I presented them both, at the same instant, and at the same distance, to the two opposite balls of my thermoscope.

The result of this experiment was perfectly conclusive: the bubble of spirit of wine began immediately to move towards the ball to which the *blackened* cold body was presented; indicating thereby, that that ball was more cooled by the frigorific rays which proceeded from the blackened surface, than the opposite ball was cooled by the rays which proceeded from an equal surface of naked metal, at the same temperature.

As this experiment appeared to me to be of great importance, I repeated it several times, and always with the same results; the motion of the bubble, which constituted the index of the instrument, constantly showing that the frigorific rays from the blackened surface were more powerful, in generating cold, than those which proceeded from the naked metal.

The bubble, it is true, did not move so far out of its place as it had done in the experiments in which hot bodies were presented to the balls; but this was not to be expected; for, though I had taken pains, by mixing salt with the ice, to produce as great a degree of cold as I conveniently could, yet still, the difference between the temperature of the balls and that of the bodies presented to them, was much greater when the hot bodies were used, than when the experiments were made with the cold bodies;

and it is evident, that the distance to which the bubble is driven out of its place, must necessarily be greater or less, in proportion as that difference is greater or less.

In those experiments in which the horizontal cylindrical vessels were filled with hot water, and then presented to the balls of the instrument, the temperature of the circular flat surfaces was that of 180°, while the temperature of the air of the room in which those experiments were made, and consequently that of the balls, was about 60°; the difference amounts to no less than 120 degrees of FAHRENHEIT'S scale; but, in these experiments with cold, the difference of the temperatures at the moment when the cold bodies were first presented to the instrument, did not probably amount to more than 40, or at the most 50 degrees; and, in a very few seconds, it must have been reduced to less than 30 degrees, in consequence of the freezing of the water precipitated by the air of the atmosphere, on the surface of the vessel containing the cold mixture.

This precipitation of water, by the surrounding air, was so copious, that the brilliancy of the polish of the metallic surface was almost instantly obscured by it; and the vessels were very soon covered with a thick coat of ice. These accidents, which were not to be prevented, affected in a very sensible manner the results of the experiment. The bubble, instead of remaining stationary for some time after it had reached the point of its greatest elongation, as it had done in the experiments with hot bodies, had no sooner reached that point, than it began to return back towards the place from which it had set out; and, as often as I wiped off the ice from the surface of the flat end of the vessel which was not blackened, and presented it clean and bright to the ball of the instrument, the bubble began

again to move towards the opposite side; which, by the bye, shows that ice emits a greater quantity of frigorific rays than a bright metallic surface, at the same temperature.

Having frequently observed, on presenting my hand to one of the balls of the thermoscope, that the instrument was greatly affected by the calorific rays which proceeded from it, apparently much more so than it would have been by a much hotter body, of the same quantity of surface, but of a different kind of substance, placed at the same distance, I was extremely curious to find out, whether *animal substances* do not emit calorific (and consequently frigorific) rays much more copiously than other substances; and whether living animal bodies do not emit them in greater abundance than dead animal matter.

The first experiment I made, with a view to the investigation of this particular point, was as simple as its result was striking and conclusive.

Exper. No. 21. Having procured a piece of gold-beater's skin, (which, as is well known, is one of the membranes that line the larger intestines in cattle, and is exceedingly thin,) I moistened it with water; and, applying it, while moist, to the flat circular end of one of my horizontal cylindrical vessels, it remained firmly attached to the surface of the metal, when it became dry. I now filled this vessel, and another, of equal dimensions, the end of which was not covered, with hot water, (at the temperature of 180°,) and presented them both, at the same moment, to the two balls of the thermoscope, and at the same distance.

The bubble of spirit of wine was immediately driven out of its place, to a great distance; and did not return to its former station, till after the vessel whose end was covered with gold-

beater's skin had been removed to a distance from the ball to which it was presented, which was *five times* greater than the distance at which the other vessel was placed from the opposite ball.

I was induced to conclude, from the result of this interesting experiment, that an animal substance emits 25 *times* more calorific rays, than a polished metallic surface of the same dimensions, both substances being at the same temperature.

Exper. No. 22. Having emptied both the vessels used in the last experiment, and refilled them with pounded ice and water, I now presented them again to the thermoscope, at equal distances from their respective balls.

The result of this experiment confirmed the conclusion I had been induced to draw from a former experiment of the same kind, (No. 13,) the motion of the bubble towards the vessel whose surface was covered with gold-beater's skin, showing that the rays which proceeded from that animal substance were considerably more efficacious in producing cold, than those which proceeded from the naked metal.

The radiation of cold bodies appearing to me to have been proved beyond all doubt, by the preceding experiments, I now set about to investigate a very important point, which still remained to be determined: I endeavoured to find out, whether the intensity of the action of the frigorific rays which proceed from cold bodies, or their power of affecting the temperatures of other warmer bodies, *at equal intervals of temperature,* is, or is not, equal to the intensity of the action of the calorific rays which proceed from hot bodies. To ascertain this point, I made the following very simple and decisive experiment.

Exper. No. 23. Having placed the thermoscope on a table, in

the middle of a large quiet room, at the temperature of 72° F. I presented to one of its balls, at the distance of 3 inches, the flat circular end of one of the horizontal cylindrical vessels (A) above described, with an oblique cylindrical neck, this vessel being filled with pounded ice and water; and, at the same moment, an assistant presented to the opposite side of the same ball of the thermoscope, at the same distance, (3 inches,) the flat end of the other similar and equal cylindrical vessel, (B,) filled with warm water at the temperature of 112° F. the opposite ball of the thermoscope being hid and defended, by means of screens, from the actions of the bodies presented to the other ball, as also from the calorific rays which proceeded from the bodies of the persons present.

From this description it appears, that while one of the balls of the thermoscope was so defended by screens that it could not be sensibly affected by the radiations of the neighbouring bodies, the other ball was exposed to the simultaneous action of two equal bodies, at equal distances; (two vertical metallic disks, 3 inches in diameter, placed on opposite sides of the ball, at the distance of 3 inches;) one of these bodies being at the temperature of 32° F. or 40 degrees below that of the ball, while the other was at 112° F. or 40 degrees above the temperature of the ball.

I knew, from the results of former experiments, that this ball would, at the same time, be heated by the calorific rays from the hot body, and cooled by the frigorific rays from the cold body; and I concluded, that if its mean temperature should remain unchanged under the influence of these two opposite actions, that event would be a decisive proof of the equality of the intensities of those actions.

The result of the experiment showed, that the intensities of

those opposite actions were in fact equal; the bubble of spirit of wine, which, by its motion, would have indicated the smallest change of temperature in the ball of the thermoscope, to which the hot and the cold bodies were presented, remained at rest.

On removing the cold body a little farther from the ball, to the distance of $3\frac{1}{2}$ inches, for instance, the hot body remaining in its former station, at the distance of 3 inches, the bubble began immediately to move towards the opposite ball of the thermoscope, indicating an increase of heat in the ball exposed to the actions of the hot and the cold bodies; but, when the hot body was removed to a greater distance, the cold body remaining in its place, the bubble indicated an increase of cold.

The celerity with which the ball of the thermoscope acquired heat, or cold, might be estimated by the velocity with which the bubble of spirit of wine advanced, or retired, in its tube; but, on the most careful and attentive observation, I could not perceive that it moved faster when the ball was acquiring heat, than when it was acquiring cold; provided that the hot and the cold bodies, from which the calorific and frigorific rays proceeded, were at the same relative distances.

From these experiments, which I lately repeated at Geneva, in the presence of Professor PICTET, Mons. de SAUSSURE, M. SENEBIER, and several other persons, we may venture to conclude, that, *at equal intervals of temperature*, the rays which generate cold, are just as real, and just as intense, as those which generate heat; or, that their actions are equally powerful, in changing the temperatures of neighbouring bodies.

On a superficial view of this subject, it might appear extraordinary, that so important a fact as that of the frigorific radiations of cold bodies should have been so long unnoticed, while the

calorific radiations of hot bodies have been so well known; but, if we consider the matter with attention, our surprise will cease. Those radiations by means of which the temperatures of neighbouring bodies are gradually changed and equalized, are not sensible to our feeling, unless the intervals of temperature be very considerable; and the constitution of things is such, that while we are often exposed to the influence of bodies heated several thousand degrees (as measured by the thermometer) above the mean temperature of the surface of the skin, it is very seldom that we have opportunities of experiencing the effects of the radiations of bodies much colder than ourselves; and we have no means of producing degrees of cold which bear any proportion to the intense heats excited by means of fire.

From the result of the experiment of which an account has just been given, it is evident, that we should be just as much affected by the calorific rays emitted by a cannon bullet at the temperature of 160 degrees of Fahrenheit's scale, (=64 degrees above that of the blood,) as by the frigorific rays of an equal bullet, ice cold, placed at the same distance; and that a bullet at the temperature of freezing mercury, could not affect us much more sensibly, by its frigorific rays, than an equal bullet at the temperature of boiling water would do, by its calorific rays; but, at these comparatively small intervals of temperature, the radiations of bodies are hardly sensible, and could never have been perceived, much less compared and estimated, without the assistance of instruments much more delicate than our organs of feeling. Hence we see how it happened, that the frigorific radiations of cold bodies remained so long unknown. They were suspected by Bacon; but their existence was first ascertained

by an experiment made at Florence, towards the end of the seventeenth century. And it is not a little curious, that the learned academicians who made that experiment, and who made it with a direct view to determine the fact in question, were so completely blinded by their prejudices respecting the nature of heat, that they did not believe the report of their own eyes; but, regarding the reflection and concentration of cold (which they considered as a negative quality) as *impossible*, they concluded, that the indication of such reflection and concentration, which they observed, must necessarily have arisen from some error committed in making the experiment.

Happily for the progress of science, the matter was again taken up, about twenty years ago, by Professor Pictet; and the interesting fact, which the Florentine academicians would not discover, was put beyond all doubt. But still, this ingenious and enlightened philosopher did not consider the appearances of a reflection of cold, which he observed in his experiments, as being *real;* nor was he led by them to admit the existence of frigorific emanations from cold bodies, analogous to those calorific emanations from hot bodies, which he calls radiant heat. He every where speaks of the reflection of cold (by metallic mirrors) as being merely *apparent;* and it is on that supposition, that the explanation he has given of the phenomena is founded.

On a supposition that the *caloric* of modern chemists has any real existence, and that heat, or an increase of temperature in any body, is caused by an *accumulation* of that substance in such body, the reflection of cold would indeed be impossible; and the supposition that such an event had taken place, would be absurd, and could not be admitted, however striking and convincing the

appearances might be which indicated that event. But, to return from this digression.

Having found that the intensity of the calorific rays emitted by a hot body, at any given temperature, depends much on the surface of such body,—that a polished metallic surface, for instance, throws off much fewer rays than the same surface, at the same temperature, would emit, if painted, or blackened in the smoke of a lamp or candle, I was desirous of finding out, whether the frigorific rays from cold bodies are affected in the same manner, by the same means, and in the same degree.

It was to ascertain that point, that the experiment No. 20 was made; and, although the result of that experiment afforded abundant reason to conclude, that those substances which, when hot, throw off calorific rays in the greatest abundance, actually throw off great quantities of frigorific rays, when they are cold; yet, as the relative quantities of these rays could not be exactly determined by that experiment, in order to ascertain so important a fact, I had recourse to the following simple contrivance.

Exper. No. 24. Having found, by the result of the last experiment, (No. 23,) that the calorific emanations of a circular disk of polished brass, 3 inches in diameter, at the temperature of 112° F. were just counterbalanced by the frigorific emanations of an equal disk of the same polished metal, at the temperature of 32° F. placed opposite to it, so that one of the balls of the thermoscope placed between these two disks, at equal distances, was just as much heated by the one as it was cooled by the other, I now blackened the two disks, by holding them over the flame of a wax candle, and repeated the experiment with them, so blackened.

I knew, from the results of former experiments, that the intensity of the calorific radiations from the hot disk, would be very much increased, in consequence of its surface being blackened; and I was certain, that if the intensity of the frigorific radiations of the cold disk should not be increased in *exactly the same degree*, the ball of the thermoscope, exposed to the simultaneous actions of these two disks, could not possibly remain at the same constant temperature, that of 72°.

The result of the experiment was very decisive: the bubble of spirit of wine remained at rest; which proved, that the intensities of the rays emitted by the two disks, still continued to be equal at the surface of the ball of the thermoscope, which, at equal distances, was exposed to their simultaneous action.

Hence we may conclude, that those circumstances which are favourable to the copious emission of calorific rays from the surfaces of hot bodies, are equally favourable to a copious emission of frigorific rays from similar bodies, when they are cold.

But it is time to consider these emanations in a new point of view. What difference can there be between calorific rays, and frigorific rays? Are not the same rays either calorific, or frigorific, according as the body at whose surface they arrive is hotter, or colder, than that from which they proceed?

Let us suppose three equal bodies, A, B, and C, (the globular bulbs of three mercurial thermometers, for instance,) to be placed, at equal distances, (3 inches,) in the same horizontal line; and let A be at the temperature of freezing water, B at the temperature of 72° F. and C at that of 102° F. The rays emitted by B will be *calorific*, in regard to the colder body A; but, in respect to the hotter body C, they will be frigorific; and,

from the results of the two last experiments, we have abundant reason to conclude, that they will be just as efficacious in heating the former, as in cooling the latter.

Before I proceed to give an account of the experiments which were made with a view to determine the relative quantities of rays emitted from the surfaces of various substances, from living animals, dead animal matter, &c. (which I must reserve for a future communication,) I shall lay before the Society the results of several experiments, of various kinds, which were made with a view to the farther investigation of the radiations of hot and of cold bodies, and of the effects produced by them.

Exper. No. 25. Having found, from the results of the experiments No. 21 and No. 22, that great quantities of rays are thrown off from the surface of the animal substance used in those experiments, (gold-beater's skin,) I now covered the whole of the external surface of one of my large cylindrical passage thermometers (No. 4) with that substance; and, filling it with boiling hot water, exposed it to cool gradually in the air of a large quiet room, in the manner often described in former parts of this Paper; another similar *naked* standard instrument (No. 3) being filled with hot water at the same time, and exposed to cool in the same situation.

The temperature of the air of the room being $51\frac{1}{2}°$, the instruments were found to cool through the standard interval of 10 degrees, namely, from $101\frac{1}{2}$ to $91\frac{1}{2}$, in the following times.

No. 4, *covered* with gold-beater's skin, in $27\frac{3}{4}$ minutes.

No. 3, which was *naked*, - - in 45 minutes.

Exper. No. 26. Being desirous of finding out whether or not the covering of animal matter, which had so remarkably facilitated the cooling of the instrument No. 4, would be equally

efficacious in facilitating the passage of heat *into* the instrument, I suffered both instruments to remain in the cold room all night; and, entering the next morning, at half an hour past seven o'clock, I found the temperature of the water in the *naked* instrument, No. 3, to be $50\frac{1}{8}°$: that in the instrument No. 4, which was covered with gold-beater's skin, was $49\frac{1}{4}°$; while the air of the room was at 48°.

At $7^h\ 30^m$ A. M. I removed both instruments into a warm room, and observed the times of their acquiring heat to be as expressed in the following Table.

Times when the observations were made.	Observed Temperature. No. 3, *naked.*	No. 4, *covered.*	Temperature of the air of the room.
At $7^h\ 30^m$	$50\frac{1}{8}°$	$49\frac{1}{4}°$	64°
7 45	$51\frac{1}{2}$	$51\frac{1}{2}$	$64\frac{1}{2}$
8 —	$52\frac{1}{2}$	$53\frac{1}{8}$	65
8 15	$53\frac{3}{4}$	$54\frac{7}{8}$	—
8 30	$54\frac{3}{8}$	56	—
8 45	$55\frac{1}{2}$	$57\frac{1}{8}$	—
9 —	$56\frac{1}{4}$	$58\frac{1}{2}$	—
9 30	$57\frac{1}{2}$	60	—
10 —	$58\frac{1}{4}$	$61\frac{1}{4}$	—
10 30	$59\frac{1}{2}$	$62\frac{1}{8}$	—
11 —	$60\frac{1}{2}$	63	—
11 30	61	$63\frac{1}{2}$	$64\frac{1}{2}$

The results of this experiment, and of several others similar to it, showed, in a manner which appeared to me to be perfectly conclusive, that those substances which part with heat with the greatest facility, or celerity, are those which also acquire it most readily, or with the greatest celerity.

If we might suppose that the temperatures of bodies are changed, not by the rays they *emit*, but by those they *receive* from other neighbouring bodies, this fact might easily be explained; but, without stopping to form any hypothesis for the explanation of these appearances, I shall proceed in my account of the various attempts I have made to elucidate, by new experiments, those parts of this interesting subject which still appeared to be enveloped in obscurity.

As the cooling of hot bodies is so much accelerated by covering their surfaces with such substances as emit calorific rays in great abundance, or with such as are much affected by the frigorific rays of the colder bodies by which they are surrounded, it seems to be highly probable, that a comparatively small part of the heat, which a body so cooled actually loses, is acquired by the air; a much greater proportion of it passing off through that *transparent* fluid, under the form of calorific rays, without affecting its temperature.

If this supposition should turn out to be well founded, the knowledge of the fact would enable us to explain several interesting phenomena, and particularly that most curious process by means of which living animals preserve an equal temperature, notwithstanding the vast quantities of heat that are continually generated in the lungs, and notwithstanding the great variations which take place in the temperature of the air in which they live.

It is evident, that the greater the power is which an animal possesses of *throwing off* heat from the surface of his body, independently of that which the surrounding air takes off, the less will his temperature be affected by the occasional changes of

temperature which take place in the air; and the less will he be oppressed by the intense heats of hot climates.

It is well known that *negroes*, and people of colour, support the heats of Tropical climates much better than white people. Is it not probable that their *colour* may enable them to throw off calorific rays with great facility, and in great abundance; and that it is to this circumstance they owe the advantage they possess over white people, in supporting heat? And, even should it be true, that bodies are cooled, not in consequence of the rays they emit, but by the action of those frigorific rays they receive from other colder bodies, (which I much suspect to be the case,) yet, as it has been found by experiment, that those bodies which emit calorific rays in the greatest abundance, are also most affected by the frigorific rays of colder bodies, it is evident, that in a very hot country, where the air and all other surrounding bodies are but very little colder than the surface of the skin, those who by their colour are prepared and disposed to be cooled with the greatest facility, will be the least likely to be oppressed by the accumulation of the heat generated in them by respiration, or of that excited by the sun's rays.

With a view to throw some light on this interesting subject, I made the following experiments.

Exper. No. 27. Having covered the flat ends of both my horizontal cylindrical vessels with gold-beater's skin, I painted one of these coverings (of this animal substance) black, with Indian ink; and then, filling both vessels with boiling hot water, I presented them, at equal distances, to the two opposite balls of the thermoscope.

The bubble of spirit of wine was immediately driven out of its

place, by the superior efficacy of the calorific rays which proceeded from the blackened animal substance.

On repeating this experiment a great number of times, and when the water in the vessels was at different degrees of temperature, (the temperature being the same in the two vessels, in each experiment,) the results uniformly indicated, that calorific rays were thrown off from the *black* surface, in greater abundance than from the equal surface which was not blackened.

Although the results of these experiments appeared to me to be so perfectly conclusive as to establish the fact in question, beyond all possibility of doubt, yet, in so interesting an enquiry, I was desirous, by varying my experiments, to bring, if possible, a variety of proofs, to support the important conclusions which result from it.

Exper. No. 28. Having covered the two large cylindrical vessels, No. 3 and No. 4, with gold-beater's skin, I painted one of them black, with Indian ink; and, filling them both with boiling hot water, I exposed them to cool, in the manner already often described, in the air of a quiet room.

No. 4, which was *blackened*, cooled through the standard interval of 10 degrees in $23\frac{1}{2}$ minutes; while the other, No. 3, which was not blackened, took up 28 minutes, in cooling through the same interval.

In a former experiment, (No. 25,) the instrument No. 4, covered with gold-beater's skin, but not blackened, had taken up $27\frac{3}{4}$ minutes, in cooling through the given interval, as we have before seen.

The results of these experiments do not stand in need of illustration; and I shall leave to physicians and physiologists to determine what advantages may be derived from a knowledge of the

facts they establish, in taking measures for the preservation of the health of Europeans who quit their native climate to inhabit hot countries.

All I will venture to say on the subject is, that were I called to inhabit a very hot country, nothing should prevent me from making the experiment of blackening my skin, or at least of wearing a black shirt, in the shade, and especially at night; in order to find out if, by those means, I could not contrive to make myself more comfortable.

Several of the savage tribes which inhabit very cold countries, besmear their skins with oil; which gives them a shining appearance. The rays of light are reflected copiously from the surface of their bodies. May not the frigorific rays, which arrive at the surface of their skin, be also reflected, by the highly polished surface of the oil with which it is covered?

If that should be the case, instead of despising these poor creatures for their attachment to a useless and loathsome habit, we should be disposed to admire their ingenuity, or rather to admire and adore the goodness of their invisible guardian and instructor, who teaches them to like, and to practice, what he knows to be useful to them.

The Hottentots besmear themselves, and cover their bodies, in a manner still more disgusting. They think themselves *fine*, when they are besmeared and dressed out according to the loathsome custom of their country. But who knows whether they may not in fact be *more comfortable*, and better able to support the excessive heats to which they are exposed? From several experiments which I made, with a view to elucidate that point, (of which an account will be given to this Society at some future period,) I have been induced to conclude, that the Hottentots

derive advantages from that practice, exactly similar to those which negroes derive from their black colour.

It cannot surely be supposed, that I could ever think of recommending seriously to polished nations, the filthy practices of these savages. That is very far indeed from being my intention; for I have ever considered cleanliness as being so indispensably necessary to comfort and happiness, that we can have no real enjoyment without it; but still, I think that a knowledge of the physical advantages which those savages derive from such practices, may enable us to acquire the same advantages, by employing more elegant means. A knowledge of the manner in which heat and cold are excited, would enable us to take measures for these important purposes with perfect certainty: in the mean time, we may derive much useful information, by a careful examination of the phenomena which occasionally fall under our observation.

If it be true, that the black colour of a negro, by rendering him more sensible to the few frigorific rays which are to be found in a very hot country, enables him to support the great heats of Tropical climates without inconvenience, it might be asked, how it happens that he is able to support, naked, the direct rays of a burning sun?

Those who have seen negroes exposed naked to the sun's rays, in hot countries, must have observed that their skins, *in that situation*, are always very shining. An oil exudes from their skin, which gives it that shining appearance; and the polished surface of that oil reflects the sun's calorific rays.

If the heat be very intense, sweat makes its appearance at the surface of the skin. This watery fluid not only reflects very

powerfully the calorific rays from the sun, which fall on its polished surface, but also, by its evaporation, generates cold.

When the sun is gone down, the sweat disappears; the oil at the surface of the skin retires inwards; and the skin is left in a state very favourable to the admission of those feeble frigorific rays which arrive from the neighbouring objects.

But I shall refrain from pursuing these speculations any farther at present.

I shall now proceed to give an account of several experiments, of various kinds, which were made with a view to a farther investigation of the radiations of cold bodies.

Having found, by several of the foregoing experiments, that the radiations of cold bodies affected my thermoscope very sensibly, even when placed at a considerable distance from it, and in situations where currents of cold air could not be suspected to exist, I was desirous of finding out, whether the cooling of a hot body would or would not be *sensibly* accelerated by those rays. To determine that point, I made the following experiment.

Exper. No. 29. Having provided two conical vessels, made of thin sheet brass, each 4 inches in diameter at the base, and 4 inches high, ending above in a cylindrical neck, 0.88 of an inch in diameter, I enclosed each of them in a cylinder of thin pasteboard, covered with gilt paper, and then covered them up with rabbit-skins, which had the hair on them, in such a manner that no part of these vessels, except their flat bottoms, was exposed naked to the air. I then covered their bottoms with gold-beater's skin, painted black with Indian ink, in order to render them as sensible as possible to calorific and frigorific rays.

This being done, I suspended these two vessels, in an erect position, or with their bottoms downwards, to the two opposite horizontal arms of a wooden stand, provided for the experiment; and I placed under each of them a pewter platter, blackened on the inside, by holding it over a lighted wax candle.

Each of these platters was 12 inches in diameter; and they were supported on the top of two shallow earthen dishes, each of which was $11\frac{1}{2}$ inches in diameter at its brim; these earthen dishes being supported on circular wooden stands, 10 inches in diameter.

A circular piece of thick drawing-paper, $12\frac{1}{2}$ inches in diameter, with a circular hole in its centre, just 6 inches in diameter, was placed on each of the platters, and served as a perforated cover to it.

The stands on which the platters were supported, were of such a height, that the upper surface of the flat bottom of each of the platters was elevated just 40 inches above the level of the floor of the room; and the horizontal arms of the wooden stand, which supported the conical vessels, was of such a height, that the flat bottoms of these vessels (which were placed perpendicularly over the centres of the platters) were just 4 inches above the flat horizontal surface of the bottoms of the platters.

One of the platters was at the temperature of the air of the room, (63° F.) but the other was kept constantly ice-cold, during the whole of the time the experiment lasted, by means of pounded ice and water, which was put into the earthen dish, over which, or rather in which, this platter was placed.

Each of the platters was just 1 inch deep, measured from the level of the top of its brim to the level of the upper surface of

the flat part of its bottom: this flat part was about 8 inches in diameter.

The two conical vessels were now filled with boiling hot water, and the times of their cooling were carefully observed.

From the above description of the apparatus used in this experiment, it is evident, that the vessel which was suspended over the ice, could not be reached by any streams of cold air that might be occasioned by that ice, or by the cooled sides of the vessel which contained it; for the air which, coming into contact with the sides of that vessel, was cooled by it, becoming specifically heavier than it was before, naturally descended, and spread itself out on the floor of the room; and the perforated circular sheet of paper, which was laid down horizontally on the platter, effectually prevented any of the air so cooled from being thrown upwards against the bottom of the conical vessel, (placed immediately over the platter,) by any occasional undulation of the air in the room.

To preserve the air of the room in a state of perfect quietness, not only the doors and windows, but even the window-shutters of the room, were kept shut; so much light only being admitted occasionally, as was necessary to observe the thermometers which were placed in the conical vessels.

In order to guard still more effectually the bottoms of the vessels which were cooling, from the effects of occasional undulations in the air of the room, over each of these vessels there was drawn a cylindrical covering of very fine thin post paper; the lower open end of which projected just half an inch below the horizontal level of the flat bottom of the vessel. These cylindrical coverings of post paper were made to fit, as exactly as

possible, the cylinders of pasteboard by which the sides of the conical vessels were covered, and defended from the air; and the warm coverings of fur (rabbit-skins) were put over all.

To confine the heat still more effectually, a quantity of eider-down had been introduced between the outside of each conical vessel, and its cylindrical neck, and the inside of the hollow cylinder of pasteboard in the axis of which it was fixed and confined.

The result of this experiment was very conclusive. The conical vessel which was suspended over the *ice-cold* pewter platter, cooled through the standard interval of 10 degrees, (namely, from the point of 50 degrees to that of 40 degrees above the temperature of the air of the room,) in 33 minutes and 42 seconds; whereas, the other vessel, which was not over ice, required 39 minutes and 15 seconds, to cool through the same interval.

Exper. No. 30. On repeating this experiment the next day, the air of the room still remaining at 63°, the times of cooling through the given interval were as follows.

	Min.	Sec.
The vessel suspended over the ice-cold platter, in	33	15
The other vessel, in - - - - -	39	30

From the results of these experiments (which were made with the greatest possible care) it appears, that the radiations of cold bodies act on warmer bodies, *at a distance*, and gradually diminish their temperatures.

It will likewise be evident, when we consider the matter with attention, that the cooling of the vessel which was suspended over the ice-cold platter, was in fact considerably more accelerated by the frigorific radiations from that cold surface than it appears to have been, when we estimate the effects produced

simply by the difference of the times taken up in the cooling of the two vessels, without having regard to any other circumstance.

These times are, no doubt, inversely as the velocities of cooling; but, as all the heat lost by the vessels, during the time of their cooling, did not pass off through their flat bottoms, and as the rays from the cold surface fell on the *bottom only* of the vessel which was suspended over it, without at all affecting its covered sides, the velocity with which the heat made its way through the covered sides of the vessels was the same in both; consequently, more heat must have passed that way, and of course less through the bottom of the vessel, when the time of cooling was the longest, that is to say, in the vessel which was not placed over ice.

As the cooling of these vessels is a complicated process, I will endeavour to elucidate the subject still farther.

As the two conical vessels were of the same form and dimensions, and contained equal quantities of hot water, the quantities of heat they parted with, in being cooled the same number of degrees, must of course have been equal.

Expressing that quantity by the algebraic symbol a, and putting $x =$ the quantity of heat which passed off through the covered sides of the vessel which was suspended over ice, during the time it was cooling through the given interval of 10 degrees, and $y =$ the quantity which passed off through the covered sides of the other vessel, during the time that vessel was cooling through the same interval; the quantity of heat which passed off through the bottom of the vessel which was placed over ice, during the time it was cooling through the given interval, must

have been $= a - x$; and that which passed off through the bottom of the other vessel, during the time of its cooling through the same interval, $= a - y$.

But, as the velocities of the heat through the covered sides of both vessels must have been equal, the quantities of heat which passed off *that way* must have been as the times of cooling.

The times of cooling in the last mentioned experiment, (No. 30,) were as follows:

	Min.	Sec.	Seconds.
Of the vessel suspended over ice	33	15	= 1995
Of the other vessel - -	39	30	= 2370

x is therefore to y, as 1995 to 2370;

consequently, $x = \frac{1995y}{2370} = 0.84177y$;

And, substituting for x, its value $= 0.84177y$, the quantities of heat which passed off through the bottoms of the two vessels, in the experiment in question, (No. 30,) must have been $= a - 0.84177y$, for the vessel which was suspended over ice; and $= a - y$, for the other vessel.

And, as y is greater than $0.84177y$, consequently $a - 0.84177y$ is greater than $a - y$, or the quantity of heat which passed off *through the bottom* of the vessel which was cooled the most rapidly, was greater than that which passed off *through the bottom* of the other vessel; and hence we perceive, that the effect produced by the frigorific rays from the cold surface, in the experiments in question, was *greater* than it appeared to be, at first sight, when it was estimated by the times of cooling.

To determine exactly *how much* the cooling was accelerated by the presence of the cold body, it is necessary to find out how much heat actually passed off through the bottoms of the two vessels, in the experiments in question. This we will endeavour

to do, by comparing the results of those experiments, with the results of some other experiments of a similar nature.

In the experiment No. 28, a cylindrical vessel of thin sheet brass, 4 inches in diameter, and 4 inches in height, covered with gold-beater's skin painted black with Indian ink, being filled with hot water, and exposed to cool in the air of a large quiet room, cooled from the point of 50 degrees to that of 40 degrees above the temperature of the air of the room, in $23\frac{1}{2}$ minutes.

The quantity of surface by which this vessel was exposed to the cold air, was = 74.5581 superficial inches, exclusive of its neck, which was well covered up with fur.

The quantity of surface which was exposed to the air, in the foregoing experiments with the conical vessels, or the area of the bottom of each of the vessels, was $(4 \times 3.14159) = 12.4263$ superficial inches.

As the diameters and heights of the conical and cylindrical vessels were equal, the contents of the former must have been to the contents of the latter as 1 to 3; and the quantities of heat which they lost in cooling were as their contents.

If now the cylindrical vessel lost a quantity of heat = 3, in $23\frac{1}{2}$ minutes, it would have disposed of a quantity = 1, (equal to that which the conical vessel lost,) in one-third part of that time, or in 7 minutes and 50 seconds.

But the quantity of surface exposed to the air in the experiment with the cylindrical vessel, was to that so exposed in the experiment with the conical vessel, as 74.5581 to 12.4263, or as 6 to 1.

Now, as the time in which any given quantity of heat can pass out of any closed vessel, into, or through, any cold fluid medium by which the vessel is surrounded, must be inversely as the

surface of the vessel, other things being equal; if a quantity of heat = 1 could pass out of the cylindrical vessel in 7 minutes and 50 seconds, it would require 6 times as long, or 47 minutes, to pass out of the conical vessel, *through its flat bottom*, supposing no heat whatever to escape through the covered sides of that vessel.

If now the whole of the heat which the conical vessel actually lost, would have required 47 minutes to have passed through the bottom of that vessel, it is evident that the quantity which actually passed through that surface, in the experiment in question, (No. 30,) could not have been, to the whole quantity actually lost, in a greater proportion than that of the times, or as $39\frac{1}{2}$ to 47.

Assuming any given number, as 10000, for instance, to represent the whole of the heat lost in the experiment, we can now determine what part or proportion of it passed off through the bottom of the conical vessel, and consequently how much of it must have made its way through its covered sides.

If the whole quantity, = 10000, would have required 47 minutes to have passed through the bottom of the vessel, the quantity which actually passed through that surface in $39\frac{1}{2}$ minutes, could not possibly have amounted to more than 8404, $= a - y$.

For it is 47 min. to 10000, as $39\frac{1}{2}$ min. to 8404. The remainder of the heat, = 10000 – 8404 = 1396 parts, $(= y,)$ must have made its way through the covered sides of the vessel.

And, if a quantity of heat = 1396, required $39\frac{1}{2}$ minutes to make its way through the covered sides of one of the conical vessels, the quantity which made its way through the covered sides of the other in $33\frac{1}{4}$ minutes, could not have amounted to

more than 1175 parts; and the remainder of that which was actually disposed of in the experiment, $= 10000 - 1175 = 8825$, $(= a - x,)$ must have passed off through the bottom of the instrument.

Hence it appears, that the quantity of heat which actually passed off through the bottom of the conical vessel which was placed over ice, in $33\frac{1}{4}$ minutes, was to that which passed off in $39\frac{1}{2}$ minutes, through the bottom of the other vessel, as 8825 to 8404; and consequently, that the velocity with which the heat passed through the bottom of the vessel which was exposed to the frigorific rays from the surface of the cold platter, was to the velocity with which it passed through the bottom of the other vessel, in the compound ratio of 8825 to 8404, and of $39\frac{1}{2}$ to $33\frac{1}{4}$; or as 10000 to 8025, which is as 5 to 4, very nearly.

From these experiments and computations it appears, that the cooling of the hot body which was placed over the ice-cold platter, was sensibly, and very considerably, accelerated by the vicinity of that cold body; may we not venture to say,—by the frigorific rays which proceeded from it?

I made several other experiments, similar to those just described, and with similar results; but I shall not take up the time of the Society, by giving a detailed account of them. I may perhaps, at a future time, find occasion to mention some of them more particularly.

In the two last-mentioned experiments, as the conical vessels were suspended in an erect position, and had a circular band or hoop of fine post paper, by which the lower end of each of them was surrounded, and which projected downwards half an inch below the horizontal level of the bottom of the vessel, and as the air which came into immediate contact with the bottom of the

vessel, and received heat from it, (though it became specifically lighter than it was before,) could not make its escape *upwards*, into the atmosphere, being confined and prevented from moving upwards by the thin projecting hoop of paper, there is no doubt but that the time of cooling was prolonged by this arrangement; for, there being much reason to believe that the propagation of heat downwards, in air, from one particle of that fluid to another, is either quite impossible, or so extremely slow as to be imperceptible, as a succession of fresh particles of cold air was prevented from coming into contact with the bottoms of the vessels, but very little heat could have been given off *immediately* to the air in those experiments.

In order to be able to form some probable conjecture respecting the quantity so given off, in cases where the succession of fresh particles of air is free and uninterrupted, I made the following experiment.

Exper. No. 31. The two conical vessels used in the last experiment, (which I shall now distinguish by calling the one No. 5, and the other No. 6,) being left suspended in the air, to the two horizontal arms of their wooden stand, at the height of 44 inches above the floor of the room, (the pewter platters, the earthen dishes, and the stands on which they were placed being removed,) both the vessels were again filled with boiling hot water, and exposed to cool in the air.

The vessel No. 5 remained in a vertical position, or with its flat bottom in a horizontal position, as before; but the vessel No. 6 was now reclined, so that its axis, and consequently the plane of its flat bottom, made an angle with the plane of the horizon, of 45 degrees. In this position of the vessel No. 6, it is evident that the air, heated by coming into contact with its

bottom, had full liberty to escape *upwards*, and to make way for other particles of colder air to come into contact with the hot surface, and be heated, rarefied, and forced upwards, in their turns; and, under these circumstances, it might reasonably be expected, that as much heat as possible would be communicated *immediately* to the air, by the hot body; and that the heat so communicated would of course accelerate the cooling of that vessel.

It was in fact cooled in a shorter time than the other, No. 5, which was suspended in a vertical position; but the difference of the times of cooling was very small; which indicates, if I am not mistaken, that a comparatively small quantity of the heat a hot body loses, when it is cooled in air, is communicated to that fluid; much the greater part of it being sent off through the air, to a distance, in calorific rays.

The vessel No. 5 was found to cool through the standard interval of 10 degrees in $38\frac{1}{2}$ minutes; and No. 6, which was in a reclined position, in $37\frac{1}{4}$ minutes.

It will no doubt be remarked, that the vessel No. 5 cooled somewhat faster in this experiment than it had done in the two preceding experiments, (No. 29 and No. 30,) when it stood over a pewter platter, which (at the beginning of the experiment at least) was at the same temperature as the air of the room.

The calorific rays from the bottom of the vessel, heating the platter in some small degree, and still more perhaps the upper surface of the perforated sheet of paper which covered it, the frigorific rays from these bodies, were, on that account, somewhat less powerful in lowering the temperature of the neighbouring hot body; and the time of its cooling was consequently a little prolonged.

In one of the preceding experiments, it cooled through the given interval in $39\frac{1}{2}$ minutes, and in the other in $39\frac{1}{4}$ minutes; but, in this experiment, it took up only $38\frac{1}{2}$ minutes, in cooling through it, as we have just seen.

Supposing now, (what appears to me to be not improbable,) that all, or very nearly all, the heat lost by the instrument No. 5 passed off in rays *through* the air, we can ascertain what part of the heat lost by the instrument No. 6, was communicated *to the air* which came into contact with its surface.

Putting the total quantity of heat lost by each of the instruments, in cooling through the given interval, = 10000; as we have just seen that a quantity of heat = 1396, passes through the covered sides of each of these instruments in $39\frac{1}{2}$ minutes, the quantities so lost in this experiment must have been as follows. By the instrument No. 5, in $38\frac{1}{2}$ minutes, = 1081; by No. 6, in $37\frac{1}{4}$ minutes, = 1046; and, deducting these quantities so lost (through the covered sides of the instruments) from the total quantity lost by each, (= 10000,) we shall find out how much heat passed off *through the bottom* of each of the instruments.

For the instrument No. 5, it is 10000 — 1.081 = 9199,
And for - - No. 6, 10000 — 1.046 = 9954.

If now the whole of the heat lost through the bottom of the instrument No. 5, passed off *through* the air in rays, as there is no reason to suppose that a less quantity passed off in the same time, *in the same way*, through the bottom of the instrument No. 6, it appears, that this last mentioned instrument must have lost, *by radiation*, or in rays which passed *through* the air, a quantity of heat = 9597.

For it is $38\frac{1}{2}$ minutes to 9919, as $37\frac{1}{4}$ minutes to 9597.

And, if of the total quantity of heat which passed off through the bottom of the conical instrument No. 6, = 9954, a quantity =9597 passed off *through* the air, in calorific rays, the remainder only, (9954 − 9597,) which amounts to no more than 357 parts, could have been communicated to the air.

Hence it would appear, that when a hot body is cooled in air, $\frac{1}{27}$ part only of the heat which it loses is acquired by the air; for 357 is to 9597, as 1 to 27, very nearly. But I shall refrain from enlarging farther on this subject at present.

One of the objects which I had in view, in the last experiment, was, to find out whether the cooling of a hot body in air, is or is not sensibly accelerated, or retarded, by the greater or lesser distance at which the body is placed from other neighbouring solid bodies, when these neighbouring bodies are at the same temperature as the air; and, as a comparison of the result of this experiment, with the results of the two preceding experiments, so strongly indicated that the cooling of the conical vessel, in the preceding experiments, had in fact been *retarded* by the vicinity of the pewter platter over which it was suspended, I was now induced to repeat these experiments with some variations.

These investigations appeared to me to be of the more importance, as I conceived that the results of them might lead to a discovery of one of the causes of the warmth of clothing.

Exper. No. 32. I now placed the pewter platters once more in their former stations, perpendicularly under the bottoms of the two conical vessels, but at the distance of 3 inches only; that which was under the vessel No. 5 being at the temperature of the air of the room, (62°,) while that placed under the vessel

No. 6, was kept ice-cold, by means of pounded ice and water, which was put into the earthen dish on the brim of which it was supported.

The times of the cooling of the vessels, through the standard interval of 10 degrees, were as follows.

No. 5 - - - in $40\frac{1}{4}$ minutes.

No. 6, which was over ice, in $33\frac{1}{4}$

Exper. No. 33. I repeated this experiment once more, but varied it, by bringing the pewter platters still nearer to the bottoms of the conical vessels. The flat horizontal part of each of the platters, was now only 2 inches below the flat surface of the bottom of the conical vessel which was suspended over it. Both the platters still remained covered by their flat circular perforated covers of paper; but it should be remembered, that the circular hole in the centre of each of these covers was no less than 6 inches in diameter, and consequently, that a large portion of the flat part of the bottom of the platter was in full view (if I may use that expression) of the bottom of the vessel which was suspended over it.

The times of cooling, in this experiment, were as follows.

No. 5 cooled through the given interval in $42\frac{3}{4}$ minutes.

No. 6, which was over ice - - in $32\frac{1}{2}$ minutes.

The results of these experiments show, (what indeed might have been expected, especially on a supposition that the heating and cooling of bodies is effected by means of radiations,) that although the cooling of the hot body suspended over a surface kept constantly cold by artificial means, was accelerated by being brought nearer to that cold surface, yet, in a case where the cold surface was less intensely cold, and where its tempera-

ture could be sensibly raised by the calorific rays from the hot body, the cooling of the hot body was retarded by a nearer approach of that cold surface.

From the results of these experiments we may safely conclude, that if the hot body, instead of being a conical vessel covered up on all sides except its flat bottom, had been a globe, and if this hot globe had been suspended in the centre of another larger thin hollow sphere, (this last being, at the beginning of the experiment, at the same temperature as the air and walls of the room,) the vicinity of the surface of this hollow globe, to the surface of the hot body, would have retarded the cooling of the hot body, in the same manner as the cooling of the conical vessel No. 5 was retarded in the foregoing experiments; and if, instead of inclosing the hot body in the centre of a single hollow sphere, of any given thickness, it were placed in the common centre of a number of much thinner concentric spheres, of different diameters, the time of cooling would be still more retarded.

By tracing the various operations which would take place in the cooling of the hot body, in this imaginary experiment, we shall become acquainted with the nature of those which actually take place, when the cooling of a hot body is prolonged by means of warm clothing.

From the results of several of the foregoing experiments we may conclude, that, supposing the thin concentric hollow spheres in which the hot body is confined to be made of metal, the cooling will be slower, if the surfaces of these spheres are polished, than if they are unpolished, or blackened: and hence we might very naturally be led to suspect, (what is probably true in fact,) that the *warmth* of any kind of substance used as

clothing, or its power of preventing our bodies from being cooled by the influence (frigorific radiations) of surrounding colder bodies, depends very much *on the polish of its surface.*

If, with the assistance of a microscope, we examine those substances which supply us with the warmest coverings, such for instance as furs, feathers, silk, &c. we shall find their surfaces not only smooth, but also very highly polished; we shall also find that, other circumstances being equal, those substances are the warmest which are the finest, or which are composed of the greatest number of fine polished detached threads or fibres.

The fine white shining fur of a Russian hare, is much warmer than coarse hair; and fine silk, as spun by the silk-worm, is warmer than the same silk twisted together into coarse threads; as I found by actual experiments, an account of which has already been laid before this Society, and published in the Philosophical Transactions.

I formerly considered the warmth of natural and artificial clothing, as depending *principally* on the obstacle it opposes to the motions of the cold air by which the hot body is surrounded; but, by a patient and careful examination of the subject, I have been convinced, that the efficacy of radiation is much greater than I had supposed it to be.

From the result of the experiment No. 31, we might be led to conclude, that a very small part only of the heat which a hot body appears to lose when it is cooled in air, is in fact communicated to that fluid; a much greater portion of it being communicated to other surrounding bodies at a distance; and, in one of my former experiments, a hot body was cooled, though it was placed in a TORRICELLIAN vacuum.

These researches appear to me to be the more interesting, as I have long been of opinion, that it must be by experiments of this kind, (showing in what manner the temperature of bodies are affected reciprocally, at different degrees of temperature, and at different distances,) that the hypothesis of radiation must be established, or proved to be unfounded.

When I speak of heat as being communicated to air *immediately* by a hot body which is cooled in it, I mean only, that it is not first communicated to other neighbouring bodies, and then given *by them* to the particles of air with which they happen to be in contact. In this last mentioned way, much of the heat, no doubt, which a hot body loses when cooled in air, is ultimately communicated to that fluid.

I am far from supposing that the particles of air which, coming into contact with a hot body, are heated in consequence of that near approximation, receive heat in any other *manner* than that in which other bodies, at a greater distance, receive it. If, in the one case, it be generated, or excited, by the agency of calorific rays, or undulations, caused by the hot body, it must, I am persuaded, be excited in the same manner in the other.

The reason why the particle of air which is in immediate contact with a hot body is heated, while other particles, near it, are not affected by the calorific rays from the hot body, which are continually passing by them, through the air, is, I conceive, because the particle heated is at *the surface of the fluid,* (air,) where these rays are either reflected, refracted, or absorbed; but, when a ray has once passed the surface of a transparent fluid, it proceeds straight forwards, without being farther affected by it, *and consequently without affecting it,* till it comes to the confines of the medium, or to the surface of some other body.

If this hypothesis of the communication, or rather *generation*, of heat, and of cold, by radiation, be true, it will enable us to explain, in a satisfactory manner, what has been called the *non-conducting power* of transparent fluids, with respect to heat; for, if heat be really communicated, or excited, in the manner above described, it is quite evident that *a perfectly transparent fluid* can receive heat only at its surface; and consequently, that heat cannot be propagated in such a fluid, by communication, from one particle of the fluid to another.

By a *transparent* fluid, I mean such an one as admits the calorific and frigorific rays, emitted by hot and by cold bodies, to pass freely through it, without obstructing their passage, or diminishing their intensities.

Whether any of the fluids with which we are acquainted be *perfectly* transparent in this sense of the word, or not, I will not pretend to say; but there is reason to think that pure water, and air, and most other fluids which are transparent to light, possess a high degree of transparency, in regard to calorific and frigorific rays; or that they give a very free passage to them, when they have once passed their surfaces.

An even or polished surface has been found to facilitate very much the reflection of the rays of light. May it not, in all cases, have an equal tendency to facilitate the reflection of calorific and frigorific rays?

In the experiments with the large cylindrical vessels, where they were exposed *naked* to cool in the air, their surfaces were polished, and they were a long time in cooling. But, when the surface of the vessel was blackened, or covered with other substances, the vessel was found to cool much more rapidly.

A large proportion of the frigorific rays from the surrounding

colder bodies were, in the former case, reflected at the polished surface of the metallic vessel; but, in the latter case, more of them were absorbed.

When a large drop of water rolls about, without being evaporated, upon the flat surface of a piece of red-hot iron, the surface of the drop is *polished;* and, the calorific rays being mostly reflected, the water is very little heated, notwithstanding the extreme intensity of the heat of the iron, and its nearness to the water.

If the iron be *less hot,* the water penetrates the pores of the oxide which covers the metal,—the drop ceases to have a polished surface,—acquires heat very rapidly,—and is soon evaporated.

If a drop of water be placed on the clean and polished surface of a metal not so easily oxidable as iron, it will retain its spherical form and polished surface, under a lower degree of temperature than on iron; and consequently will be less heated, and less rapidly evaporated by a moderate heat.

If a large drop of water be put carefully into a clean silver spoon, previously heated very hot, (that is to say, so hot as to give a loud hissing noise when touched with the wetted finger, but much below the heat of red-hot metal,) the drop will support, or rather *resist,* this heat for a considerable time; but, after the spoon has been suffered to cool down nearly to the temperature of boiling water, a drop of water put into it will be evaporated instantaneously.

It appears, from the results of these experiments, to be probable that, under high temperatures, air is attracted by metals so much more strongly than water, that even the weight of a drop of water is not sufficient to force away the stratum of air

which covers, and adheres to, the surface of a metal on which the drop reposes; but, at lower temperatures, this does not seem to be the case.

The following experiment, which I made several months ago, with a view to investigate the cause of the slow evaporation of drops of water placed on hot metals, will, I think, throw much light on this subject.

Exper. No. 34. Taking a clean polished silver spoon, I blackened the inside of it, by holding it over the flame of a wax candle; then, putting a large drop of water into it, I found, as I expected, that the drop took a spherical form, and rolled about in the spoon, without wetting its blackened surface.

I now held the spoon over the flame of a candle, and attempted to make the water boil; but I found it to be absolutely impossible. The handle of the spoon became so very hot, that I could not hold it in my hand without being burnt, though it was wrapped up in three or four thicknesses of linen; but still the drop of water did not appear to be at all affected by this intense heat. If the bowl of the spoon were touched with the finger, a hissing noise announced that it was extremely hot; but still, the water remained perfectly quiet in the spoon, without being evaporated.

Having in vain attempted to make this drop of water boil, and not being able to hold the spoon over the flame of the candle any longer, on account of the heat of its handle, I now poured the drop into the palm of my hand. I found it to be warm, but by no means scalding hot.

By holding the spoon, with a pair of tongs, over the flame of the candle for a longer time, I found that a drop of water in

the spoon gradually *changed its form*, became less, and was at length evaporated: from being spherical and lucid, it gradually took an oblong form, and its surface became obscure; and, when it was evaporated, it left a kind of skin behind it, which was evidently composed of the particles of black matter, which had by degrees attached themselves to its surface, and which probably had contributed not a little to its being at last heated, and evaporated.

The change in the form of the drop of water, and more especially the gradual loss of its lucid appearance, made me suspect that it had turned round during the experiment. If it really did so, its motion must either have been extremely rapid, or very slow; for, though I examined it with great attention, I could not perceive that it had any rotatory motion.

I will take the liberty to mention another little experiment, which I have often made, to amuse myself and others, though it may perhaps be thought too trifling to deserve the attention of the Royal Society.

Exper. No. 35. If a large drop of water be formed at the end of a small splinter of light wood, (deal, for instance,) and this drop be thrust quickly into the centre of the flame of a newly snuffed candle, which burns bright and clear, the drop of water will remain for a considerable time in the centre of the flame, and surrounded by it on every side, without being made to boil, or otherwise apparently affected by the heat; and, if it be taken out of the flame, and put upon the hand, it will not be found to be scalding hot.

If it be held for some time in the flame, it will be gradually diminished, by evaporation; but there is much reason to think,

that the heat which it acquires is not communicated to it by the flame, but by the wood to which it adheres, which is soon heated by the flame, and even set on fire.

I cannot refrain from just observing, that it appears to me to be extremely difficult to reconcile the results of any of the foregoing experiments, with the hypothesis of modern chemists respecting the *materiality of heat*.

Deeply sensible of the insufficiency of the powers of the human mind, to unfold the mysteries of nature, and discover the agents she employs, and their mode of action, in her secret and invisible operations; and being moreover fully aware of the danger of forming an attachment to a false theory, and of the folly of wasting time in idle speculations; I have ever, in my philosophical researches, been much more anxious to discover new facts, and to show how the discoveries of others may be made useful to mankind, than to invent plausible theories; which much oftener tend to misguide, than to lead us in the path of truth and science.

There are however situations, in which an experimental enquirer sometimes finds himself, where it is almost impossible for him to abstain from forming, or adopting, some general theory, for the purpose of explaining the phenomena which fall under his observation, and directing him in his future researches.

Finding myself in that situation at this time, I beg the attention, and above all the *indulgence*, of the Society, while I endeavour to explain the conjectures I have formed, respecting the nature of heat, and the mode of its communication.

Hot and *cold*, like *fast* and *slow*, are mere relative terms; and, as there is no relation, or proportion, between motion and a state of rest, so there can be no relation between any degree

of heat and absolute cold, or a total privation of heat: hence it is evident, that all attempts to determine the place of *absolute cold,* on the scale of a thermometer, must be nugatory.

It seems probable that *motion* is an essential quality of matter; and that rest is no where to be found in the universe.

We well know, that all those bodies which fall under the cognizance of our senses are in motion; and there are many appearances which seem to indicate, that the constituent particles of all bodies are also impressed with continual motions among themselves; and that it is these motions (which are capable of augmentation and diminution) that constitute the *heat* or temperature of sensible bodies.

The only effects of which we have any idea, resulting from the action of one body on another, are a change of velocity, or a change of direction, or both. We perceive, it is true, that certain bodies have a power of affecting certain other bodies *at a distance;* but this is no proof that the effects produced are essentially different from those which result from collision; for, if an elastic body be interposed between the two bodies, their actions on each other may be communicated through such intermediate elastic body, which, when the action is at an end, and the effects resulting from it on the two bodies have taken place, will be in the same state precisely in which it was before the action began.

If a bell, or any other solid body, *perfectly elastic,* placed in a perfectly elastic fluid, and surrounded by other perfectly elastic solid bodies, were struck, and made to vibrate, its vibrations would, by degrees, be communicated, by means of the undulations, or pulsations they would occasion in the elastic fluid medium, to the other surrounding solid and elastic bodies. If

these surrounding bodies should happen to be already vibrating, and with the same velocity as that with which the bell is made to vibrate by the blow, the undulations in the elastic fluid, occasioned by the bell, would neither increase nor diminish the velocity or frequency of the vibrations of the surrounding bodies; neither would the undulations caused by the vibrations of these bodies tend to accelerate, or to retard, the vibrations of the bell. But, if the vibrations of the bell were more frequent than those of the surrounding bodies, the undulations it would occasion in the elastic fluid, would tend to accelerate the vibrations of the surrounding bodies: on the other hand, the undulations occasioned by the slower vibrations of the surrounding bodies, would retard the vibrations of the bell; and the bell, and the surrounding bodies, would continue to affect each other, until, by the vibrations of the latter being gradually increased, and those of the former diminished, in consequence of their actions on each other, they would all be reduced to the same *tone*.

Supposing now, that heat be nothing more than the motions of the constituent particles of bodies among themselves, (an hypothesis of ancient date, and which always appeared to me to be very probable,) if for the bell we substitute a hot body, the cooling of it will be attended by a series of actions and reactions, exactly similar to those just described.

The rapid undulations occasioned in the surrounding ethereal fluid, by the swift vibrations of the hot body, will act as calorific rays on the neighbouring colder solid bodies; and the slower undulations, occasioned by the vibrations of those colder bodies, will act as frigorific rays on the hot body; and these reciprocal actions will continue, but with decreasing intensity, till the hot body, and those colder bodies which surround it, shall, in

consequence of these actions, have acquired the same temperature, or until their vibrations have become isochronous.

According to this hypothesis, *cold* can with no more propriety be considered as the absence *of heat,* than a low or grave sound can be considered as the absence of a higher or more acute note; and the admission of rays which generate cold, involves no absurdity, and creates no confusion of ideas.

On a superficial view of the subject, it may perhaps appear difficult to reconcile solidity, hardness, and elasticity, with those never-ceasing motions which we have supposed to exist among the constituent particles of all bodies; but a patient investigation of the matter will show, that the admission of that supposed fact, instead of rendering it more difficult to form distinct and satisfactory ideas of the causes on which those qualities of bodies depend, will rather facilitate those abstruse researches.

Judging from all the operations of nature, of the causes of which we are able to form any distinct ideas, we are certainly led to conclude, that the force of dead matter, (and perhaps of living matter also,) or its power of affecting, that is to say, of *moving*, other matter, or of *resisting its impulse*, depends on its motion.

If, therefore, solid (or fluid) bodies have any powers whatever, either of impulse or of resistance, it appears to me to be more reasonable to ascribe them to the living forces residing in them, —to the never-ceasing motions of their constituent particles,— than to suppose them to be derived from their want of power, and their total indifference to motion and to rest.

No reasonable objection against this hypothesis, (of the incessant motions of the constituent particles of all bodies,) founded on a supposition that there is not room sufficient for these

motions, can be advanced; for we have abundant reason to conclude, that if there be in fact any indivisible solid particles of matter, (which however is very problematical,) these particles must be so extremely small, compared to the spaces they occupy, that there must be ample room for all kinds of motions among them.

And, whatever the nature or directions of these internal motions may be, among the constituent particles of a solid body, as long as these constituent particles, in their motions, do not break loose from the systems to which they belong, (and to which they are attached by gravitation,) and run wild in the vast void by which each system is bounded, (which, as long as the known laws of nature exist, is no doubt impossible,) the form or external appearance of the solid cannot be sensibly changed by them.

But, if the motions of the constituent particles of any solid body be either increased or diminished, in consequence of the actions, or radiations, of other distant bodies, this event could not happen without producing some visible change in the solid body.

If the motions of its constituent particles were *diminished* by these radiations, it seems reasonable to conclude, that their elongations would become less, and consequently, that the volume of the body would be contracted; but, if the motions of these particles were increased, we might conclude, *a priori*, that the volume of the body would be expanded.

We have not sufficient data to enable us to form distinct ideas of the nature of the change which takes place when a solid body is melted; but, as fusion is occasioned by heat, that is to say, by

an augmentation (from without) of that action which occasions expansion, if expansion be occasioned by an increase of the motions of the constituent particles of the body, it is, no doubt, a certain additional increase of those motions, which causes the form of the body to be changed; and, from a solid, to become a fluid substance.

As long as the constituent particles of a solid body which are at the surface of that body, do not, in their motions, *pass by each other,* the body must necessarily retain its form or shape, however rapid those motions or vibrations may be; but, as soon as the motion of these particles is so augmented that they can no longer be restrained, or retained within these limits, the regular distribution of the particles, which they acquired in crystallization, is gradually destroyed; and the particles so detached from the solid mass, form new and independent systems, and become a liquid substance.

Whatever may be the figures of the orbits which the particles of a liquid describe, the mean distances of those particles from each other remain nearly the same as when they constituted a solid, as appears by the small change of specific gravity which takes place, when a solid is melted, and becomes a liquid; and, on a supposition that their motions are regulated by the same laws which regulate the solar system, it is evident that the additional motion they must necessarily acquire, in order to their taking the fluid form, cannot be lost, but must continue to reside in the liquid, and must again make its appearance, when the liquid changes its form, and becomes a solid.

It is well known that a certain quantity of *heat* is requisite to melt a solid; which quantity disappears, or remains *latent* in the

liquid produced in that process; and that the same quantity of heat reappears, when this liquid is congealed, and becomes a solid body.

But, before I proceed any farther in these abstruse speculations, I shall endeavour to investigate some of the consequences which would necessarily result from the radiations of hot and of cold bodies, supposing those radiations to exist, and their motions and actions to be regulated by certain assumed laws.

And first, it is evident that the intensity of the rays emitted by a luminous point, in a perfectly transparent medium, is every where as the squares of the distance from that point, inversely; for the intensity of those rays must be as their condensation; and their condensation being diminished, in proportion as the space they occupy is increased, if we suppose all the rays which proceed in all directions from any point, to set out at the same instant, and to move with the same velocity, in right lines, these simultaneous rays (or undulations) will, in their progress, form a sphere, which sphere will increase continually in size, as the rays advance; and, as all the rays must be found at the surface of this sphere, their intensity, or condensation, must necessarily be as the surface of the sphere, inversely, or as the squares of the distance, inversely, from the centre of the sphere, or, which is the same thing, from the luminous point from which these rays proceed; the surfaces of spheres being to each other as the squares of their radii.

Supposing now, (what indeed appears to be incontrovertible,) that the intensity of the rays which hot and cold bodies emit, in a medium perfectly transparent, follows the same law, we can determine what effects must be produced, by the largeness, or

smallness, of the confined space (of a room, for instance) in which a hot body is placed, to cool.

To simplify this investigation, we will suppose this confined space to be a hollow sphere of ice, 9 feet in diameter, at the temperature of freezing water; and the hot body to be a solid sphere of metal, 2 inches in diameter, at the temperature of boiling water, placed in the centre of it; and we will suppose farther, that this hollow sphere is void of air, and that the cooling of the hot body is effected solely by the frigorific rays from the ice.

The question to be determined is, in what manner the cooling of the hot body would be affected, by increasing the diameter of this hollow sphere of ice?

Let us suppose its diameter to be increased to 18 feet. Its internal surface will then be to the surface of a sphere 9 feet in diameter, as the square of 18 to the square of 9, that is to say, as 324 to 81, or as 4 to 1. And, as the quantity of frigorific rays emitted are, *cæteris paribus*, as the surface from which they proceed, the quantity of rays emitted by the internal surface of the larger sphere, will be to the quantity emitted by the internal surface of the smaller, as 4 to 1.

But the intensities of these rays, at the common centre of these spheres, (where the hot body is placed,) being as the squares of the distances from the radiating points, inversely, the intensity of the rays from the internal surface of the smaller sphere, must be to the intensity of the rays from the internal surface of the larger sphere, as 4 to 1, at the common centre of those spheres.

Now, as the time of the cooling of the hot body will depend

on the *quantity* of frigorific rays which arrive at its surface, and on the *intensity* of their action; and, as the intensity of the rays from the internal surface of the sphere, at its centre, is diminished in the same proportion as the surface of the sphere is augmented when its diameter is increased; it follows, that a hot body placed in the centre of a hollow sphere, at any given constant temperature below that of the hot body, will be cooled in the same time, or with the same celerity, whatever may be the size of the sphere.

If this conclusion be well founded, (and I see no reason to suspect that it is not so,) it will follow, from the principles assumed, that the hot body will be cooled in the same time, in whatever part of the hollow sphere it be situated. And, as the cooling of the body is not affected, that is to say, accelerated, or retarded, either by the greater or smaller size of the inclosed space in which it is confined, or by its situation in that confined space, so it cannot be in any manner affected, either by the form of that hollow space, or by the presence of a greater or less number of other solid bodies; provided always, that all these surrounding bodies be at the same constant temperature.

If, however, any of these surrounding bodies, the temperature of which is liable to be sensibly changed during the experiment, by the calorific rays emitted by the hot body, be placed *very near* that body, the cooling of that hot body will be retarded; the rays from this neighbouring body, *so heated*, being less frigorific than those from other bodies at a greater distance, which it intercepts.

The results of all my experiments on the cooling of bodies, tended uniformly to confirm the above conclusions.

Admitting that the cooling of a hot body is effected solely by

the rays which proceed from colder bodies, and that these rays, like those of light, are reflected, refracted, and concentrated, according to certain known laws, by the polished surfaces of mirrors and lenses, it might perhaps be imagined, that the cooling of a hot body might be accelerated, or retarded, by giving it some peculiar form; or by placing near it, and in certain positions with respect to it, two or more highly polished reflecting mirrors.

As these conjectures, if well founded, might lead to experiments from the results of which the truth or falsehood of the hypothesis in question might be demonstrated, it is of much importance that this matter should be thoroughly investigated. I shall therefore beg the indulgence of the Society, while I endeavour to examine it with that careful attention which it appears to me to deserve.

When different solid substances, heated to the same degree of temperature, are exposed in the air to cool, those among them which appear to the touch to be the hottest, are not those which cool the fastest, or which send off calorific rays, through the air, in the greatest abundance.

As polished metals reflect a great part of the rays from other bodies which arrive at their surfaces, and as they are neither heated nor cooled by the rays so reflected, their temperatures are slowly changed by the actions of the surrounding bodies at a different temperature.

When a hot polished metallic body is exposed in the air to cool, surrounded by other bodies at the same temperature as that of the cold air, as most of the rays from the surrounding bodies are reflected at the polished surface of the hot body, it is evident that two sorts of rays must proceed from the surface of

that body, namely, those calorific rays which that hot body emits, and those other rays (which with regard to the surrounding bodies are neither calorific nor frigorific) which it reflects.

On a cursory view of the subject, one might be led to imagine, that, as the rays which proceed from the hot metallic body are of two kinds, the energy of the calorific rays, which properly belong to the hot body, might be diminished by those other reflected rays by which they are accompanied, and with which they may be said to be mixed; but, a more careful examination of the matter will show that this cannot be the case; that is to say, as long as all the surrounding bodies continue to be at the same temperature. If the temperature of the surrounding bodies be different, such of them will be affected, by the reflected rays, as happen to be of a temperature different from that from which the ray originated; but still, the effects produced by the rays emitted by the hot body, will be the same, or their power of effecting changes in the temperatures of other (hotter or colder) bodies, will remain undiminished, and unchanged.

The reason why their effects are not more powerful than they are found to be, is not because they are mixed with other reflected rays, but because they are few; the greater part of the rays which the hot body actually emits being reflected, and turned back upon itself, by the reflecting surface by which it is immediately surrounded.

That the reflecting surface at which the rays of light are turned back and reflected, which impinge against the polished surface of any solid or fluid body, is actually situated *without the body*, and even at some distance from it, has been proved by the most decisive experiments; and there are so many striking

analogies between the rays of light and those invisible rays which all bodies, at all temperatures, appear to emit, that we can hardly doubt of their motions being regulated by the same laws.

Perhaps there may be no other difference between them, than exists between those vibrations in the air which are audible, and those which make no sensible impression on our organs of hearing.

If the ear were so constructed that we could hear all the motions which take place in the air, we should, no doubt, be stunned with the noise; and, if our eyes were so constructed as to see all the rays which are emitted continually, by day and by night, by the bodies which surround us, we should be dazzled and confounded by that insupportable flood of light, poured in upon us on every side.

Taking it for granted that these invisible radiations exist, we will endeavour to trace the effects which must necessarily be produced by them, in order to see if these investigations will not lead us to a discovery of the causes of some appearances which have hitherto been enveloped in much obscurity.

Suppose two concave reflecting mirrors, of highly polished metal, each 18 inches in diameter, and 18 inches focal distance, to be placed opposite to each other, at the distance of 10 feet, in a large quiet room, in which the air, and the walls of the room, remain constantly at the same temperature, (that of freezing water, for instance,) without any variation.

If we suppose the floor, ceiling, walls of the room, and doors and windows, to be lined with a covering of ice, at the temperature of freezing water, we can then, without any difficulty, conceive that the temperature of the room may remain the same,

notwithstanding the presence of hotter bodies, which are brought into it for the purpose of making experiments.

Let us now suppose one of the mirrors to be at the temperature of freezing, and the other at that of boiling water; and let us see what effects they would produce, on each other, by their radiations.

And first, with respect to the hot mirror, it is evident that it will be cooled, not only by the frigorific rays which proceed from the cold metal of which the opposite mirror is constructed, but also by such of the frigorific rays from the sides of the room as, impinging against the polished reflecting surface of the cold mirror, and being reflected by that surface, happen to fall on the surface of the hot mirror, without being reflected by it.

But, as the quantity of rays which the cold mirror *reflects* is greater, in proportion as the reflecting surface is more perfect, while the quantity of rays emitted by this cold mirror is less, in proportion as its reflecting surface is more perfect, it is extremely probable that the *total* quantity of frigorific rays (emitted and reflected) which, coming from the surface of the cold mirror, impinge against the surface of the hot mirror, will be the same, whatever may be the degree of polish, or reflecting power, of the cold mirror. And, if this be the case, we may conclude, that the presence of this mirror will have no effect whatever on the hot mirror; or, that it will no more expedite its cooling than any other body, of any other form, would do, at the same distance, and occupying the same space.

It might perhaps be imagined, that the *form* of the cold mirror might concentrate the rays it emits and reflects, and, by such concentration, produce a greater effect on the opposite mirror than if its surface were flat, or of any other form; but a more

attentive examination of the matter will show, that no such concentration actually takes place: for, with regard to those rays which are *emitted* by this cold body, as they proceed from each point of its surface *in all directions*, it is perfectly evident that these are not concentrated; and, with respect to those which are *reflected*, it is equally certain that they are not concentrated; because, in order to their being concentrated, they must arrive at the surface of the mirror in parallel lines, and in the direction of the axis of the mirror, which, under the given circumstances, is evidently impossible.

Hence we see, that the presence of the cold mirror will not tend, in the smallest degree, either to accelerate, or to retard, the cooling of the hot mirror; that is to say, provided its temperature be not raised by the calorific rays from the hot mirror.

If its temperature be raised by those rays, it will tend to retard the cooling of the hot mirror; but, even in this case, it will not retard it more than any other polished metallic body would do, of any other form, having the same area, or quantity of surface opposed to the hot mirror, and being placed at the same distance from it.

By a similar train of reasoning, it may be shown, that the *form* of the hot body (that of a concave mirror) will contribute nothing to the effect it will produce on the cold mirror, in heating it, by the calorific rays it emits; and that it will itself be cooled neither faster nor slower, on account of its peculiar form.

Let us now suppose both mirrors to be at the temperature, precisely, of the room, (that of freezing water;) and, that a bullet, or other small body of a spherical form, at the temperature of boiling water, be placed in the focus of one of the mirrors; which mirror we shall call A.

As the rays emitted by this hot body are sent off in right lines, in all directions, in the same manner as light is emitted by luminous bodies; all those rays which fall on the concave polished surface of the mirror A, will be reflected (as is well known) in lines nearly parallel to the axis of the mirror; they will consequently fall on the concave polished surface of the opposite mirror B; and, being there again reflected, they will be *concentrated* at the focus of the second mirror.

If now a sensible thermometer, at the temperature of the room, be placed in this focus, it will immediately begin to rise, in consequence of the heat generated in it by the action of these calorific rays, so accumulated in that place.

If, instead of being placed in the focus of this second mirror, the thermometer be placed at a very small distance from that focus, on one side of it, the instrument, however sensible it may be, will not be apparently affected by the rays from the hot body.

This experiment, which is of ancient date, has often been made, and always with the same results.

Let us now suppose the hot body to be removed from the focus of the mirror A; and that a colder body be substituted in place of it. And, in the first place, we will suppose the temperature of this colder body to be that of freezing water, or just equal to that which reigns in the room.

As the rays which bodies at the same temperature send off from one to the other, have no tendency to increase, or to diminish, the temperature of those bodies, the concentration of rays in the focus of the mirror B, proceeding from the ice-cold body placed in the focus of the mirror A, can have no effect on a

thermometer, at the same temperature, which is exposed to their action.

If heat be a vibratory motion of the constituent particles of bodies, and if the rays which sensible bodies send off in all directions be undulations in an ethereal elastic fluid by which they are surrounded, occasioned by those motions; as the pulsations in this fluid must be isochronous with the vibrations by which they are occasioned, these pulsations or undulations can neither accelerate nor retard the vibrations of other bodies at the surfaces of which they arrive; provided the vibrations of the constituent particles of such bodies are, at that time, isochronous with the vibrations of the constituent particles of the body from which these undulations proceed. But, to return to our experiment.

Suppose now that, instead of this ice-cold body, another much colder, at the temperature of freezing mercury, for instance, be placed in the focus of the mirror A, and that a thermometer at the temperature of freezing water be placed in the focus of the mirror B; what might be expected to be the result of this experiment?—That the thermometer would fall, in consequence of its being cooled by the accumulation of frigorific rays proceeding from this very cold body.

Now this is what actually happened, in the celebrated experiment of my ingenious friend Professor Pictet, of Geneva.

Several attempts have been made to explain the result of that experiment, on the supposition that caloric has a real or material existence, and that radiant heat is that substance, emitted and sent off in right lines, in all directions, from the surfaces of hot bodies. But none of these explanations appear to me to be

satisfactory. One of the most plausible of them, is that which is founded on a supposition that caloric is emitted continually, under the form of radiant heat, by all bodies, at all temperatures; but in greater abundance by hot bodies than by such as are colder; and that a body, at the same time that it sends off radiant caloric in all directions, to the bodies by which it is surrounded, receives it in return, in greater or less quantities, from all those bodies;—that, in all cases where a body, in any given time, receives more radiant caloric than it gives off, an accumulation of caloric in the body takes place, in consequence of which accumulation it becomes hotter;—but, when it gives off more caloric in any given time than it receives, its quantity of caloric is gradually diminished, and it becomes colder;—and, that a constant temperature results, from the quantities of caloric emitted and received continually being equal. But, besides the difficulty of explaining how, or by what mechanism, it can be possible for the same body to receive and retain, and reject and drive away, the same kind of substance, at one and the same time, (an operation not only incomprehensible, but apparently impossible, and to which there is nothing to be found analogous, to render it probable,) many other reasons might be brought to show, that this hypothesis, of the supposed continual interchanges of caloric between neighbouring bodies, is very improbable; and, among the rest, there is one which appears to me to be quite conclusive.

As the point in dispute seems to be of great importance to the science of heat, I shall endeavour to examine it with all possible attention; and, in order to put the hypothesis in question to the test, we will see if it will accord with the results of

some of the foregoing experiments; which, in order to their being more easily comprehended and examined, I shall elucidate by figures.

Let the two opposite ends of the cylinders A and B (Plate V. Fig. 4) represent the two vertical metallic disks, of equal dimensions, which were presented, at the same time, to the ball of the thermoscope C, in the experiment No. 23.

In that experiment, the disk A being at the temperature of 32° F. (that of freezing water,) and the disk B at 112° F. while the ball of the thermoscope C, and all other surrounding bodies, were at 72°, it was found, that the temperature of the thermoscope was not changed by the simultaneous actions of these two bodies, the one hot, and the other cold.

In order to account for this result, on the hypothesis before mentioned, we must begin by supposing that the ball of the thermoscope gives off radiant caloric continually, in all directions, and receives it, in return, from the surfaces of all the bodies by which it is surrounded.

With regard to all these surrounding bodies, (excepting the disks A and B,) as they are at the same temperature as the ball of the thermoscope, (that of 72°,) they will give continually to that instrument, just as much radiant caloric as they receive from it; and no change of temperature will result from these equal interchanges.

But, in respect to the disk A, as that is colder than the ball of the thermoscope, it returns to it a smaller quantity of radiant caloric than it receives from it; consequently, the thermoscope receives continually less than it gives: it would of course be gradually exhausted of caloric, and become colder, were it not

for the compensation it receives for this loss, from the disk B. This disk, being hotter than the thermoscope, gives to it continually, more radiant caloric than it receives from it; and, were it not for the simultaneous loss of caloric which the instrument sustains, in its interchanges with the cold disk A, its quantity of caloric would be augmented, and it would become hotter.

Now, as the temperature of the ball of the thermoscope is an arithmetical mean between that of the disk A and that of the disk B, it is reasonable to suppose, that the thermoscope receives just as much more caloric from B than it gives to it, as it gives to A more than it receives from it; and, if that be the case in fact, it is evident, that the simultaneous actions of the two disks on the ball of the thermoscope (or the traffic which they carry on with it in caloric) can neither tend to increase, nor to diminish, the original stock of that substance belonging to that instrument; consequently, the instrument will neither be heated, nor cooled, by these interchanges, but will continue invariably at the same constant temperature.

This explanation is plausible; but, before the hypothesis on which it is founded can be admitted, we must see if it will agree with the results of other experiments; for the greatest care ought always to be used in the admission of hypotheses in physical researches; and, in no case can it be more indispensably necessary, than where an hypothesis has evidently been contrived for the sole purpose of explaining a single experiment, or elucidating a new fact.

When the surface of the metallic disk B was blackened, by holding it over the flame of a candle, the intensity of its radiation,

at the given temperature, (that of 112°,) was found to be very considerably increased; and when (being so blackened) it was again presented to the ball of the thermoscope, at the same distance as in the last-mentioned experiment, and the cold disk A (at the temperature of 32°) was placed opposite to it, at an equal distance, as represented in Fig. 5, the thermoscope, instead of continuing to retain its original temperature, (that of 72°,) was now gradually heated.

There is nothing, it is true, in that event, which appears difficult to explain on the assumed principles; for, if the quantity of radiant caloric emitted by the disk B, be increased by blackening its surface, the quantity received from it by the ball of the thermoscope must be increased also; and that additional quantity must of course tend to raise the temperature of the instrument. But here is an experiment which cannot be explained on those principles.

The surface of the cold disk A having been blackened, as well as that of the hot disk B, when both disks (blackened) were again presented, at equal distances, to the ball of the thermoscope, as represented in Fig. 6, it was found, that the original temperature of the thermoscope remained unchanged.

The result of this most interesting experiment proves, that the ball of the thermoscope was just as much cooled by the influence of the cold blackened disk, as it was heated by the hot blackened disk.

Now, as it was found by experiment, that the intensity of the radiation of the disk B was *increased* by the blackening of the surface of that disk, we must conclude, that the intensity of the radiation of the disk A was likewise *increased* by the use

of the same means: but, if those radiations be *caloric*, emitted by those bodies, (which the hypothesis in question supposes,) how did it happen, that the ball of the thermoscope, instead of being *more heated* by the additional quantity of caloric which it received in consequence of the blackening of the disk A, was actually *more cooled?*

It may perhaps be said, by the advocates for the hypothesis in question, that the blackening of the surface of the disk A, caused a greater quantity of caloric to be sent off to it by the ball of the thermoscope. Without insisting on an explanation of the mode of action of the cause which is supposed to produce this effect, (which I might certainly do, as the supposition is perfectly gratuitous,) I will content myself with just observing, that as the surface of the opposite disk *was also blackened*, this supposed augmentation of the quantity of caloric emitted by the ball of the thermoscope, *occasioned by the blackening of the surfaces of the bodies presented to it*, can be of no use in explaining the phenomena in question.

The results of the two last mentioned experiments appear to me to be very important; and I do not see how they can be reconciled with the opinions of modern chemists, respecting the nature of heat.

In order to simplify our speculations on this abstruse subject, we have hitherto supposed, that *difference of temperature* depends solely on the difference *of the times* of the vibrations of the component particles of bodies. It is possible, however, and even probable, that it depends principally on the *velocities* of those particles: for it is easy to perceive, that the more rapid the motions of those particles are, the greater their elongations must

be, in their vibrations; and the more, of course, will the volume of the body they compose be expanded.

It is well known, that the pulsations occasioned in an elastic fluid, by the vibrations of an elastic solid body, proceed from that body in all directions; and that these pulsations are every where (that is to say, at all distances from the body) isochronous with the vibrations of the solid body; it is known also, that the mean velocity of any individual particle of the fluid is less, in proportion as the distance of the particle is greater from the centre from which these pulsations proceed.

In the case of the pulsations occasioned in the air by the vibrations of sonorous bodies, those pulsations are every where isochronous with the vibrations of the sonorous body; and the time, or *frequency* of those pulsations, determines the *note;* but it is the *velocity* of the particles of the air, or the breadth of the wave, on which the *force* or *strength* of the sound depends; and this velocity becoming less, as the distance from the sonorous body increases, the sound is weakened in the same proportion.

There are several circumstances which might lead us to suspect, that *colour* depends on the *frequency* of those pulsations which have been supposed to constitute light; and that the *heat* produced by them is in proportion to their *force*.

If this supposition should be well founded, a knowledge of that important fact might perhaps enable us to explain several very interesting phenomena;—the combustion of inflammable bodies, for instance; and the great intensity of the heat which is produced by the *concentration* of calorific rays.

There are several well known experiments with burning glasses, which show that the intensity of the heat generated by

the concentration of the solar rays, is not simply as the *condensation* of those rays, but in a higher proportion; and, that it depends much on their *direction,* being greater, as the angle is greater at which they meet at the focus of the lens.

That fact is certainly very remarkable. It has often been the subject of my meditations; and it has contributed not a little to the opinion I have been induced to adopt, respecting the nature of light and of heat. I never could reconcile it with the supposition that heat is caused by the *accumulation* of any thing *emitted* by the sun; or by any other body which sends off calorific radiations.

Reserving for a future communication, an account of the sequel of my enquiries respecting the subject which I have undertaken to investigate, I shall conclude this long Paper with some observations, concerning the *practical uses* that may be derived from a knowledge of the facts which have been established by the results of the foregoing experiments.

In all cases where it is designed to *preserve the heat* of any substance which is confined in a metallic vessel, it will greatly contribute to that end, if the external surface of the vessel be very clean and bright: but, if the object be to *cool* any thing quickly, in a metallic vessel, the external surface of the vessel should be painted, or covered with some of those substances which have been found to emit calorific rays in great abundance.

Polished tea-urns may be kept boiling hot with a much less expence of spirit of wine (burnt in a lamp under them) than such as are varnished; and the cleaner and brighter the dishes, and covers for dishes, are made, which are used for bringing

victuals on the table, and for keeping it hot, the more effectually will they answer that purpose.

Saucepans, and other kitchen utensils, which are very clean and bright on the outside, may be kept hot with a smaller fire than such as are black and dirty; but the bottom of a saucepan, or boiler, should be blackened, in order that its contents may be made to boil quickly, and with a small expence of fuel.

When kitchen utensils are used over a fire of sea-coal, or of wood, there will be no necessity for blackening their bottoms, for they will soon be made black by the smoke; but, when they are used over a clear fire made with charcoal, it will be adviseable to blacken them; which may be done in a few moments, by holding them over a wood or coal fire, or over the flame of a lamp, or candle.

Proposals have often been made for constructing the broad and shallow vessels (flats) in which brewers cool their wort, of metal; on a supposition that the process of cooling would go on faster in a metallic vessel than in a wooden vessel; but this would not be found to be the case in fact, a metallic surface being ill calculated for expediting the emission of calorific rays.

The great thickness of the timber of which brewers flats are commonly made, is a circumstance very favourable to a speedy cooling of the wort; for, when the flats are empty, this mass of wet wood is much cooled, not only by the cold air which passes over it, but also, and more especially, by evaporation; and, when the flat is again filled with hot wort, a great part of the heat of that liquid is absorbed by the cold wood.

In all cases where metallic tubes filled with steam are used

for warming rooms, or for heating drying-rooms, the external surface of those tubes should be painted, or covered with some substance which facilitates the emission of calorific rays. A covering of thin paper will answer that purpose very well, especially if it be black, and if it be closely and firmly attached to the surface of the metal with glue.

Tubes which are designed for *conveying* hot steam from one place to another, should either be well covered up with *warm* covering, or should be kept clean and bright. It would, I am persuaded, be worth while, in many cases, to gild them, or at least to cover them with what is called gilt paper, or with tin foil, or some other metallic substance which does not easily tarnish in the air.

The cylinders, and principal steam-tubes of steam-engines, might be covered, first with some warm clothing, and then with thin sheet brass, kept clean and bright. The expence of this covering would, I am confident, be amply repaid, by the saving of heat and fuel which would result from it.

If garden walls painted black acquire heat faster, when exposed to the sun's direct rays, than when they are not so painted, they will likewise cool faster, during the night; and gardeners must be best able to determine whether these rapid changes of temperature are, or are not, favourable to fruit trees.

Black clothes are well known to be very warm in the sun; but they are far from being so in the shade, and especially in cold weather. No coloured clothing is so cold as black, when the temperature of the air is below that of the surface of the skin, and when the body is not exposed to the action of calorific rays from other substances.

It has been shown, that the warmth of clothing depends much on the *polish* of the surface of the substance of which it is made; and hence we may conclude that, in choosing the colour of our winter garments, those dyes should be avoided which tend most to destroy that polish: and, as a white surface reflects more light than an equal surface, equally polished, of any other colour, there is much reason to think that white garments are warmer than any other, in cold weather. They are universally considered as the coolest that can be worn, in very hot weather, and especially when a person is exposed to the direct rays of the sun; and, if they are well calculated to reflect calorific rays in summer, they must be equally well calculated to reflect those frigorific rays by which we are cooled and annoyed in winter.

I have found, by direct and decisive experiments, (of which an account will hereafter be given to this Society,) that garments of fur are much warmer, in cold weather, when worn with the fur or hair outwards, than when it is turned inwards. Is not this a proof that we are kept warm by our clothing, not so much by confining our heat, as by keeping off those frigorific rays which tend to cool us?

The fine fur of beasts, being a highly polished substance, is well calculated to reflect those rays which fall on it; and, if the body were kept warm by the rays which proceed from it being reflected back upon it, there is reason to think, that a fur garment would be warmest when worn with the hair inwards; but, if it be by reflecting and turning away the frigorific rays from external (colder) bodies, that we are kept warm by our clothes in cold weather, we might naturally expect, that

a pellisse would be warmest when worn with the hair outwards, as I have found it to be in fact.

The point here in question is by no means a matter of small importance; for, until the principles of the warmth of clothing be understood, we shall not be able to take our measures with certainty, and with the least possible trouble and expence, for defending ourselves against the inclemencies of the seasons, and making ourselves comfortable in all climates.

The fur of several delicate animals becomes white in winter, in cold countries; and that of the bears which inhabit the polar regions, is white in all seasons. These last are exposed alternately, in the open air, to the most intense cold, and to the continual action of the sun's direct rays during several months. If it should be true that heat, and cold, are excited in the manner above described, and that white is the colour most favourable to the reflection of calorific and frigorific rays, it must be acknowledged, even by the most determined sceptic, that these animals have been exceedingly fortunate, in obtaining clothing so well adapted to their local circumstances.

The excessive cold which is known to reign, in all seasons, on the tops of very high mountains, and in the higher regions of the atmosphere, and the frosts at night, which so frequently take place on the surface of the plains below, in very clear and still weather, in spring and autumn, seem to indicate, that frigorific rays arrive continually at the surface of the earth, from every part of the heavens.

May it not be by the action of these rays that our planet is cooled continually, and enabled to preserve the same mean temperature for ages, notwithstanding the immense quantities

of heat that are generated at its surface, by the continual action of the solar rays?

If this conjecture should be well founded, we should be led to conclude, that the inhabitants of certain hot countries, who sleep at night on the tops of their houses, in order to be more cool and comfortable, do wisely, in choosing that situation to pass their hours of rest.

Fig. 2.

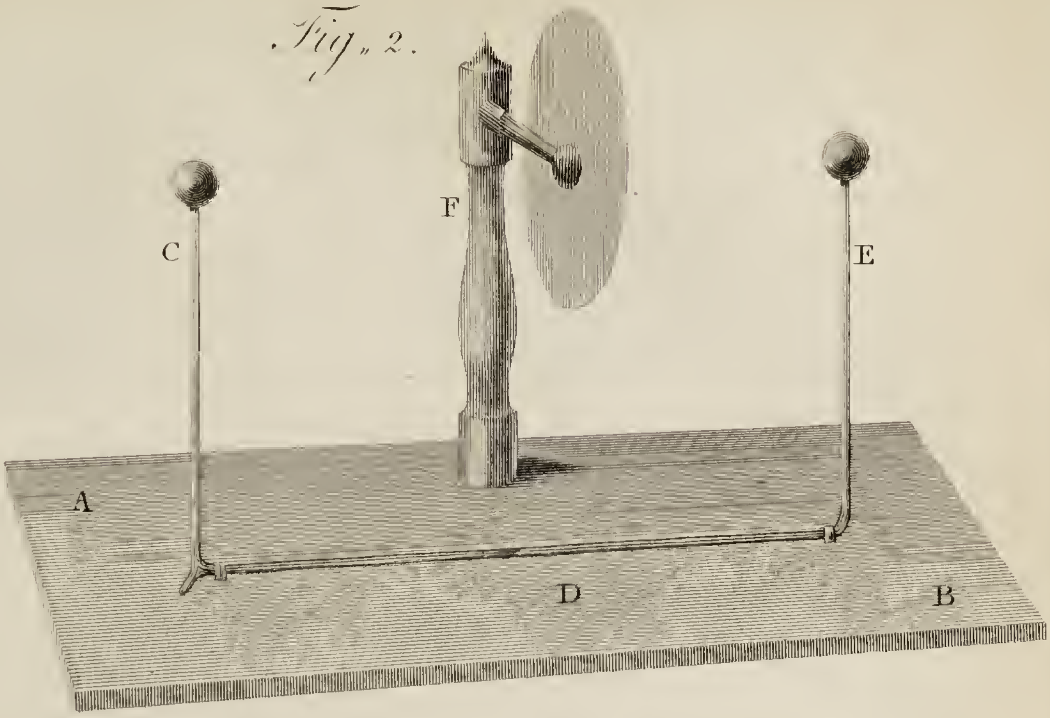


Fig. 1.

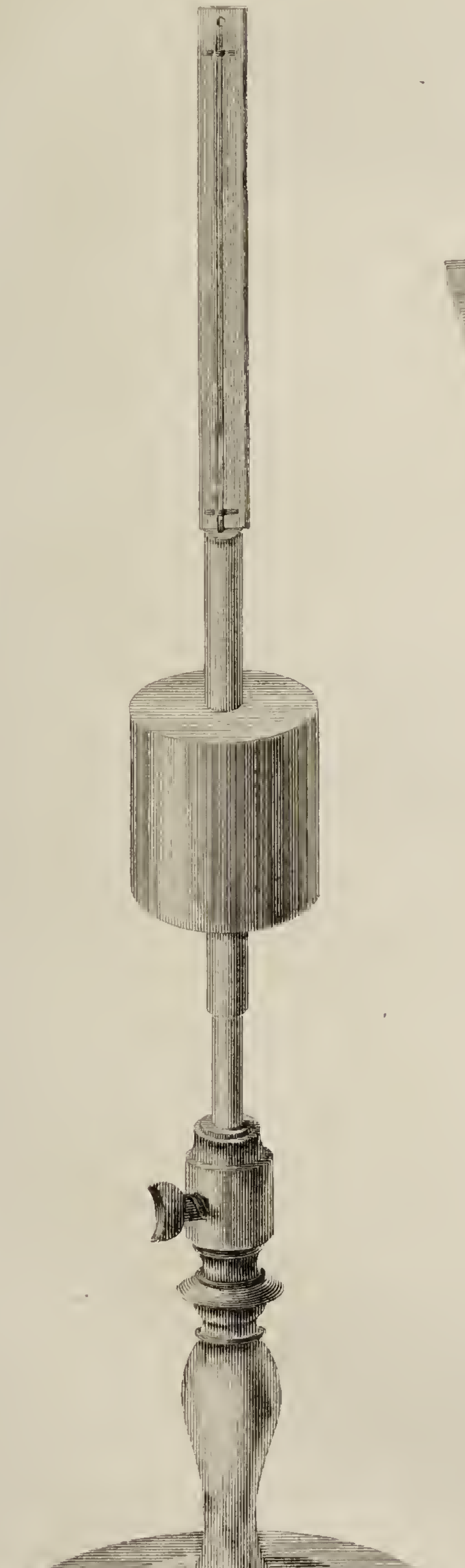
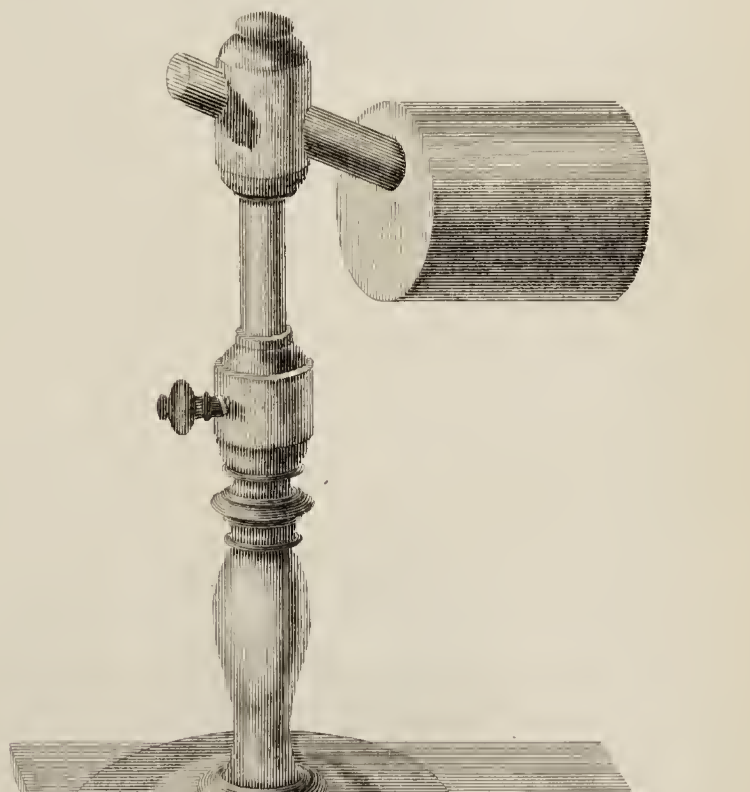


Fig. 3.



Basire sc.

Fig. 4.

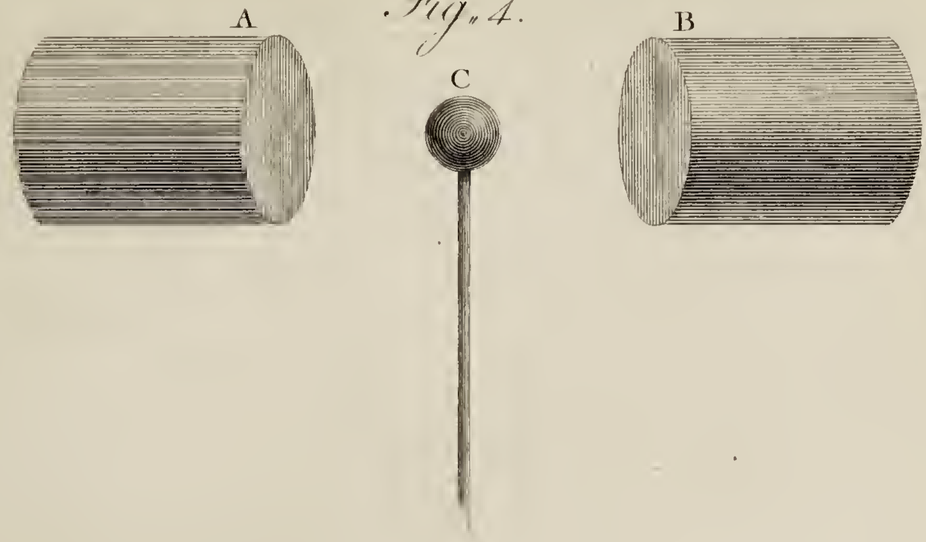


Fig. 5.

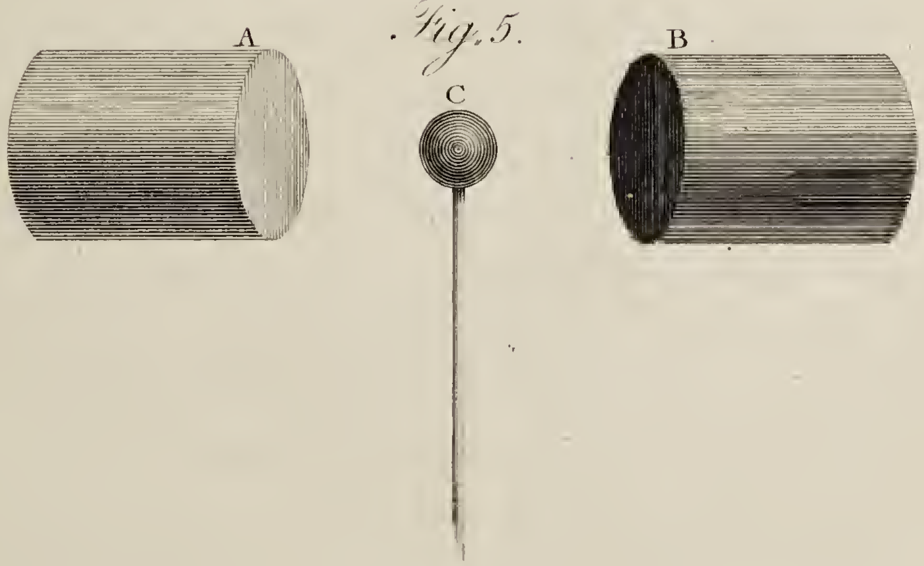


Fig. 6.

Basire sc.

VIII. *Experiments and Observations on the Motion of the Sap in Trees. In a Letter from* Thomas Andrew Knight, *Esq. to the Right Hon. Sir* Joseph Banks, *Bart. K. B. P. R. S.*

Read February 16, 1804.

MY DEAR SIR,

In the Observations on the Descent of the Sap in Trees, which I last year took the liberty to request you to lay before the Royal Society, I offered a conjecture, that the vessels of the bark, which pass from the leaves to the extremities of the roots, were, in their organization, better calculated to carry the fluids they contain towards the roots than in the opposite direction. I had not, however, at that time, any experiment directly to support this supposition; but I thought the forms generally assumed by trees in their growth, evinced the compound and contending actions of gravitation, and of an intrinsic power in the vessels of the bark, to give motion to the fluid passing through them. In the account of the experiments which I have now the honour to address to you, I trust I shall be able to adduce some interesting facts, in support of that inference.

Having selected, in the spring of 1802, four strong shoots of the vine, growing along the horizontal trellis of my vinery, I depressed a part of each shoot, whilst it was soft and succulent, about three inches deep, into the mould of a pot placed beneath it for that purpose; but without making any wound, or incision, in the young shoots thus employed as layers.

In this position they remained during the succeeding summer; and, in the autumn, had nearly filled the pots, which were ten inches in diameter, with their roots. As soon as the leaves had fallen, the layers were disengaged from the parent stocks; and about five inches of wood, containing one bud, were left, both at the proper and the inverted end of each layer. Every bud was also, by previous management, made to stand at an equal distance from the mould in the pots, and with an equal elevation, of about thirty-six degrees. About one inch of wood was likewise left at each end of every layer, beyond the buds.

In the succeeding spring, the buds vegetated strongly, both at the proper and at the inverted ends of the layers, as the experiments of Hales and du Hamel had given me reason to expect; and, in one instance, the bud at the inverted end of the layer grew with greater vigour than that at its proper end: but the growth of these buds was not the object which I had in view.

I have already stated, that nearly an inch of wood was left at each end of every layer, beyond the bud; and, to this wood, at the inverted ends of the layers, my attention was chiefly directed: for, if the vessels of the bark possessed the powers I attributed to them, I concluded that the sap would be impelled to the inverted ends of the layers, and be there employed in the production of new wood, and roots; and, in this, my expectations were not disappointed. At the proper end of the layers, the wood immediately beyond the buds became dry and lifeless, early in the succeeding summer; the stems also, between the buds and the mould in the pots, increased in size as usual; and nothing peculiar occurred. But, at the inverted end, appearances were extremely different: new wood here accumulated rapidly beyond

the buds, and numerous roots, of considerable length, were emitted, whilst no sensible growth took place between the base of the young shoots and the mould in the pots.

It having been proved by Du Hamel, that inverted parts of trees readily emit roots, I expected to derive further information from cuttings of this kind: I therefore planted, in the autumn of 1802, forty cuttings of the gooseberry-tree, and an equal number of the common currant-tree; one half of each being inverted. Of the former, not one of the inverted cuttings succeeded; whereas few of the latter failed; and in these I had an opportunity of observing the same accumulation of wood above the bases of the annual shoots, and the same mode of growth, in every respect, as in the inverted vines; except that no roots were emitted at their upper ends. The same thing occurred, without any variation, in inverted grafts of the apple-tree.

If it be admitted, according to the theory I have on a former occasion laid before you, that the sap descends from the leaves through the vessels of the bark, and that such vessels are, in their organization, better calculated to carry their contents towards the original roots than in the opposite direction, it will be extremely easy to explain the cause of the accumulation of wood, and the emission of roots, above, instead of below, the base of the annual shoots. The vessels of the bark (the *vaisseaux propres* of Du Hamel) commencing in the leaves, were formerly traced by M. Mariotte, and subsequently by myself, (being ignorant of his discovery,) to the extremities of the roots; and, when a cutting, or tree, is planted in its natural position, the sap passes downwards through these, to afford matter for new roots, and to increase the bulk of those already formed, having given proper nutriment to the branches and trunk in its descent.

But, in the inverted cutting, or tree, these vessels become inverted; and, if their organization be such as I have supposed it, a considerable part of that fluid, which naturally descends, will be carried upwards, and occasion the production of new wood, above, instead of below, the junction of the annual shoot with the older wood, as in the experiments I have described. The force of gravitation will, however, still be felt; and, by its agency, sufficient matter to form new roots may be conveyed to those parts of the inverted cutting, or tree, which are beneath the soil. Besides, if we suppose a variation to exist in the powers or organization of the vessels which carry the sap towards the root, we may also attribute, in a great measure, to this cause, the different forms which different species or varieties of trees assume; for, if the fluid in these vessels be impelled with much force towards the roots, little matter will probably be deposited in the branches, which, in consequence, will be slender and feeble, as in the vine; and there is not any tree that has been the subject of my experiments, in which new wood accumulated so rapidly at the upper end of inverted plants. To an excess of this power, in the vessels of the bark, we may also ascribe the peculiar growth of what are called weeping trees; for, by this power, the effects of gravitation will be, in a great degree, suspended; and the pendant branch will continue healthy and vigorous, by retaining its due circulation. The perpendicular branch will, however, still possess some advantages; for, in this, gravitation will act on the fluid descending from the leaves; and these will of course absorb from the atmosphere with increased activity. A greater quantity of matter will therefore enter, within any given portion of time, into vessels of the same capacity; and this increased quantity may frequently exceed

that which the vessels of the bark are immediately prepared to carry away. Much new wood will in consequence be generated, and increased vigour given; and, the same causes operating through successive seasons, will give the ascendancy we generally observe in the perpendicular branch.

In the preceding experiments, none of the layers, or cuttings, exceeded a few inches in length; and, to the summit of these the sap appeared to rise, through the inverted tubes of the wood, nearly as well as in those which retained their natural position. But some former experiments had induced me to suspect, that this would not be the case in longer cuttings; I therefore planted, in the autumn of 1802, twelve cuttings of the sallow, *(Salix caprea,)* inverting one half of them. The whole readily emitted roots, and grew with luxuriance; but their modes of growth were extremely different. In the cuttings which stood in their natural position, vegetation proceeded with most vigour at the points most elevated; but, in the inverted cuttings, it grew more and more languid as it became distant from the ground, and nearly ceased, towards the conclusion of the summer, at the height of four feet. The new wood also, which was generated by these inverted cuttings, accumulated above the bases of the annual shoots, as in the preceding instances.

These facts appear to prove, that the vessels of plants are not equally well calculated to carry their contents in opposite directions; and, I think, afford some grounds to suspect that the vessels of the bark, like those which constitute the venous system of animals, (to which they are in many respects analogous,) may be provided with valves, whose extreme minuteness has concealed them from observation.

The experiments, and still more, the Plates, of Hales, have

induced naturalists to draw conclusions in direct opposition to the preceding. But the Plates of that great naturalist are not always taken correctly from nature;* and Plates, under such circumstances, however fair and candid the intentions of an author may be, will too often be found somewhat better calculated to support his own hypothesis, than to elucidate the facts he intends to state.

The preceding peculiarities in the growth of inverted cuttings, appear to have escaped the observation of Du Hamel; and, as very few instances of error, or want of accurate observation, will ever be found in the works of that excellent naturalist, I must request permission to send you some of the subjects of my experiments, as vouchers for my own accuracy.

Of the inverted cuttings employed by Du Hamel, a small portion only appears to have remained above the ground; and, under such circumstances, the different forms of those growing in their natural, or inverted, position would be scarcely observable. It appears also, from his experiments, that such inverted cuttings, in subsequent years, grow with as much vigour as others that are not inverted; whence we must conclude, that the organization of the internal bark becomes again inverted, and adapted to the position of the branch. The growth of some inverted plants of the gooseberry-tree, which I obtained, many years ago, from layers, gave me reason to draw a different conclusion; for these always continued weak and dwarfish. I do not, however, entertain the slightest degree of doubt, but that the assertion of Du Hamel is perfectly correct.

I intended to have added some observations on the repro-

* The eleventh Plate (Vegetable staticks) is that to which, in this place, I particularly allude.

duction of buds and roots of trees; but these would necessarily extend the present Paper to an immoderate length; I shall therefore reserve them for a future communication, and conclude with an account of an experiment which more properly belongs to the Paper I had the honour to address to you last year, but which had not then succeeded.

I have stated, in that Paper, that the leaf-stalk, the fruit-stalk, and the tendril, of the vine, had been successfully substituted, in many instances, for each other; but that I had failed in my efforts to engraft a bunch of grapes, by approach, on the leaf-stalk; owing, I conceived, to the operation having been improperly performed. In those experiments, I cut the leaf-stalk into the form of a wedge, and made an incision in the fruit-stalk, adapted to receive it; but, under such circumstances, the leaf-stalk (as I had proved by many experiments) has no power to generate new matter; and the wounds of the fruit-stalk heal so slowly, that I readily anticipated the ill success of the operation. In the last spring, I pared off similar portions of the leaf-stalk and fruit-stalk; and, bringing the wounded parts into contact, I secured them closely together, by means of a bandage, letting the leaf remain. Under these circumstances, an union took place; and the fruit-stalk being then taken off below the point of junction, and the leaf-stalk above it, the grapes drew their whole nutriment through the remaining part of the leaf-stalk. They did not, however, acquire their full size; and the seeds were small, and, I think, incapable of vegetating; but this I attribute to the want of nutriment in quantity rather than in quality; for the union of the vessels of the leaf-stalk with those of the fruit-stalk was very imperfect. The grapes, which

were the purple Frontigniac, possessed their musky flavour, in the same degree with others growing on the same plant.

There is another experiment in my last Paper, which I will also notice here, because it appears to lead to some important conclusions, and had been tried only in a single instance. I have there stated, that the stem of a young tree became elliptical, by being confined to move only in the segment of a large circle. This experiment was successfully repeated, during the last year, on other trees; but I have nothing to add to the description which I have already given.

I am, &c.

T. A. KNIGHT.

METEOROLOGICAL JOURNAL,

KEPT AT THE APARTMENTS

OF THE

ROYAL SOCIETY,

BY ORDER OF THE

PRESIDENT AND COUNCIL.

METEOROLOGICAL JOURNAL

for January, 1803.

1803	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
	°	H.	M.	°	°	Inches.	°	Inches.	Points.	Str.	
Jan. 1	46	8	0	46	52	29,05	93	0,165	E	1	Cloudy.
	48	2	0	48	54	29,20	90		S	1	Fair.
2	42	8	0	42	53	29,26	93	0,345	NNE	1	Rain.
	45	2	0	45	55	29,43	90		W	1	Fair.
3	38	8	0	43	53	29,71	93		SSE	1	Cloudy.
	48	2	0	48	55	29,77	90		S	1	Cloudy.
4	38	8	0	41	53	29,69	94		E	1	Cloudy.
	45	2	0	45	54	29,61	89		E	1	Cloudy.
5	41	8	0	41	53	29,66	91		E	1	Cloudy.
	43	2	0	43	55	29,68	88		NE	1	Cloudy.
6	40	8	0	40	53	29,79	91		E	1	Cloudy.
	45	2	0	44	55	29,78	91		ESE	1	Cloudy.
7	44	8	0	44	53	29,62	93	0,215	SSE	1	Rain.
	45	2	0	45	56	29,55	93		SE	1	Rain.
8	43	8	0	43	54	29,32	93	0,400	N	1	Cloudy.
	45	2	0	45	56	29,31	88		NNE	1	Cloudy.
9	42	8	0	43	53	29,05	92	0,065	ESE	1	Cloudy.
	45	2	0	45	56	29,06	87		E	1	Cloudy.
10	34	8	0	34	52	29,12	88	0,112	E	2	Cloudy.
	35	2	0	35	53	29,21	86		E	2	Cloudy.
11	29	8	0	29	49	29,75	77		NE	2	Fair.
	31	2	0	31	52	29,88	75		NE	2	Fair.
12	24	8	0	24	47	30,00	79		NNE	1	Fine.
	32	2	0	32	50	30,00	73		NE	1	Fine.
13	22	8	0	23	45	30,03	83		NE	1	Fine.
	29	2	0	29	49	30,02	79		NE	1	Fair.
14	25	8	0	27	45	29,98	79		NE	2	Cloudy.
	30	2	0	30	48	29,92	77		NE	1	Fair.
15	28	8	0	28	46	29,88	78		NE	2	Cloudy.
	29	2	0	29	47	29,85	77		NE	2	Cloudy.
16	26	8	0	27	44	29,87	80		ENE	2	Cloudy.
	27	2	0	27	47	29,81	81		E	2	Cloudy.

METEOROLOGICAL JOURNAL

for January, 1803.

1803	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.	
		H.	M.	°	°	Inches.		Inches.	Points.	Str.
Jan. 17	26°	8	0	33	45	29,56	89°		E	2	Cloudy.
	34	2	0	35	45	29,60	95	0,037	E	1	Foggy.
18	35	8	0	35	48	29,61	95		E	1	Cloudy.
	33	2	0	33	44	29,44	91		E	1	Cloudy.
19	36	8	0	36	47	29,45	93		E	1	Rain.
	34	2	0	34	45	29,55	93		NE	1	Cloudy.
20	37	8	0	37	48	29,61	91	0,085	NE	1	Cloudy.
	43	2	0	43	46	29,77	93		SSW	1	Cloudy.
21	42	8	0	42	49	29,72	92		SSW	1	Cloudy.
	43	2	0	43	49	29,70	95		SSE	1	Cloudy.
22	41	8	0	43	47	29,50	95		SSE	2	Cloudy.
	45	2	0	44	50	29,37	89		SSE	2	Cloudy.
23	40	8	0	41	48	29,26	93	0,120	SE	1	Cloudy.
	43	2	0	43	51	29,32	93		ESE	1	Cloudy.
24	34	8	0	35	48	29,48	92		E	1	Cloudy.
	37	2	0	37	51	29,54	88		E	1	Cloudy.
25	27	8	0	27	47	29,71	83		E	2	Cloudy.
	35	2	0	35	48	29,74	83		E	2	Cloudy.
26	19	8	0	19	45	29,70	83		NE	2	Cloudy.
	22	2	0	22	46	29,66	83		NE	2	Snow.
27	22	8	0	23	43	29,66	86		N	1	Snow.
	28	2	0	27	45	29,68	81		N	1	Fair.
28	25	8	0	28	44	29,72	84		N	1	Cloudy.
	33	2	0	32	47	29,72	81		N	1	Fair.
29	26	8	0	28	44	30,01	79		NE	2	Cloudy.
	31	2	0	30	46	30,08	79		NE	1	Fair.
30	26	8	0	27	43	30,12	84		NW	1	Fair.
	36	2	0	36	46	30,11	81		NW	1	Cloudy.
31	32	8	0	33	43	30,15	89		N	1	Cloudy.
	38	2	0	38	45	30,18	86		N	1	Cloudy.

METEOROLOGICAL JOURNAL
for February, 1803.

1803	Six's Therm. least and greatest Heat.	Time. H.	Time. M.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds. Points.	Winds. Str.	Weather.
	°			°	°	Inches.	°	Inches.			
Feb. 1	34	7	0	34	44	30,28	90		NW	1	Cloudy.
	38	2	0	38	46	30,15	83		WNW	1	Fair.
2	35	7	0	36	46	29,96	91		W	1	Cloudy.
	41	2	0	41	48	29,77	82		W	1	Fair.
3	34	7	0	34	46	29,47	89		NW	1	Cloudy.
	37	2	0	37	48	29,61	86		NE	1	Cloudy.
4	27	7	0	27	46	30,17	79		NE	1	Fair.
	34	2	0	34	48	30,16	78		NE	1	Cloudy.
5	26	7	0	27	46	30,03	85		NE	1	Fair.
	35	2	0	35	49	29,96	80		N	1	Fair.
6	31	7	0	33	46	29,47	88		W	1	Snow.
	38	2	0	38	48	29,31	79		W	2	Cloudy.
7	29	7	0	29	46	29,40	86		NE	2	Snow.
	32	2	0	32	49	29,56	82		NE	1	Fair.
8	26	7	0	27	44	29,86	88		NE	1	Snow.
	32	2	0	32	46	29,97	81		NE	1	Cloudy.
9	28	7	0	28	44	30,17	88		NE	1	Cloudy.
	33	2	0	33	46	30,23	82		NE	1	Fair.
10	22	7	0	22	43	30,38	84		NE	1	Cloudy.
	32	2	0	32	48	30,42	77		S	1	Fair.
11	19	7	0	20	44	30,48	80		NE	1	Fair.
	32	2	0	32	45	30,46	81		NE	1	Cloudy.
12	20	7	0	20	42	30,46	87		NE	1	Cloudy.
	32	2	0	32	44	30,35	82		SSE	1	Cloudy.
13	31	7	0	36	43	29,98	94	0,145	S	1	Rain.
	43	2	0	42	44	29,78	95		S	1	Rain.
14	31	7	0	33	43	29,72	93	0,180	SW	1	Cloudy.
	43	2	0	41	45	29,67	92		SW	1	Cloudy.
15	39	7	0	40	44	29,44	94	0,052	SW	1	Cloudy.
	48	2	0	48	47	29,29	91		W	1	Cloudy.
16	40	7	0	40	47	29,27	78		W	1	Fair.
	46	2	0	45	51	29,41	74		W	1	Cloudy.

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1803	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H.	M.	°	°	Inches.		Inches.	Points.	Str.	
	°						°				
Feb. 17	43	7	0	46	48	29,50	90		W	2	Cloudy.
	51	2	0	51	52	29,42	86		SW	2	Cloudy.
18	42	7	0	42	51	29,48	82	0,075	SW	1	Cloudy.
	47	2	0	47	52	29,55	76		SW	1	Fair.
19	36	7	0	37	50	29,74	90		SSE	1	Cloudy.
	46	2	0	46	53	29,56	90		S	1	Rain.
20	44	7	0	44	51	29,46	88		SSE	2	Cloudy.
	49	2	0	47	54	29,44	84		SSE	2	Fair.
21	40	7	0	40	53	29,60	92	0,112	NE	1	Cloudy.
	43	2	0	43	55	29.72	92		NE	1	Cloudy.
22	38	7	0	38	52	29,99	91	0,037	SW	1	Rain.
	45	2	0	45	55	30,11	73		SW	1	Fair.
23	36	7	0	36	52	30,19	88		SSE	1	Fair.
	47	2	0	47	54	30,15	79		S	1	Fair.
24	41	7	0	43	53	30,00	87		S	2	Fair.
	48	2	0	48	55	29,90	87		S	2	Cloudy.
25	36	7	0	36	53	30,00	88	0,143	W	1	Cloudy.
	48	2	0	48	55	30,02	80		SW	1	Fair.
26	46	7	0	47	54	30,01	91		SW	1	Cloudy.
	52	2	0	52	56	29,97	83		SW	1	Cloudy.
27	50	7	0	50	55	29,86	90		W	2	Cloudy.
	53	2	0	52	59	29,91	69		W	2	Fair.
28	44	7	0	44	56	30,16	83		SW	1	Cloudy.
	51	2	0	48	56	30,04	90		SW	1	Rain.

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for March, 1803.

1803	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hy-gro-me-ter.	Rain.	Winds.		Weather.
		H.	M.	°	°	Inches.		Inches.	Points.	Str.	
	°						°				
Mar. 1	49	7	0	49	55	29,98	88	0,016	SW	2	Cloudy.
	56	2	0	56	58	29,97	82		W	2	Cloudy.
2	49	7	0	49	56	29,77	85		W	1	Cloudy.
	53	2	0	53	58	29,60	81		W	2	Cloudy.
3	36	7	0	36	54	29,38	83	0,138	NE	2	Cloudy.
	39	2	0	39	57	29,47	76		NE	2	Fair.
4	32	7	0	33	53	29,80	85		NNE	2	Cloudy.
	40	2	0	40	57	29,88	77		NE	2	Fair.
5	28	7	0	29	53	29,95	78		W	1	Cloudy.
	37	2	0	37	55	29,94	72		SSW	1	Fair.
6	31	7	0	32	52	30,02	82		E	1	Fair.
	40	2	0	40	55	30,05	75		ESE	1	Cloudy.
7	31	7	0	31	51	30,08	81		NE	2	Cloudy.
	36	2	0	36	52	30,08	72		NE	2	Cloudy.
8	33	7	0	33	49	30,08	75		NE	2	Cloudy.
	40	2	0	40	52	30,08	70		NE	2	Hazy.
9	31	7	0	31	48	30,04	80		NE	2	Cloudy.
	36	2	0	36	51	30,07	75		NE	2	Hazy.
10	31	7	0	31	49	30,02	77		NE	2	Cloudy.
	34	2	0	34	50	30,02	76		NE	2	Cloudy.
11	31	7	0	32	48	30,12	77	0,030	NE	2	Cloudy.
	37	2	0	36	51	30,17	81		NE	2	Snow.
12	26	7	0	26	48	30,34	87		NE	2	Fair.
	36	2	0	36	52	30,39	80		NE	2	Fair.
13	25	7	0	26	48	30,38	86		W	1	Fair.
	42	2	0	40	51	30,32	76		SW	1	Fair.
14	38	7	0	40	49	30,17	92		WSW	1	Cloudy.
	52	2	0	51	53	30,08	76		W	1	Cloudy.
15	44	7	0	45	50	29,75	92		SW	1	Cloudy.
	50	2	0	50	53	29,58	92		SW	1	Rain.
16	35	7	0	36	50	29,81	87	0,132	NE	1	Cloudy.
	44	2	0	44	53	29,88	78		E	1	Fair.

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1803	Six's Therm. least and greatest Heat.	Time. H.	Time. M.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds. Points.	Winds. Str.	Weather.
	°			°	°	Inches.	°	Inches.			
Mar. 17	36	7	0	39	50	29,83	85		S	2	Cloudy.
	46	2	0	46	52	29,84	86		S	1	Cloudy.
18	45	7	0	45	52	30,03	82		SSW	1	Cloudy.
	54	2	0	54	55	30,07	88		S	1	Cloudy.
19	46	7	0	48	53	30,15	90		S	2	Cloudy.
	51	2	0	51	56	30,16	87		S	2	Cloudy.
20	45	7	0	46	54	30,08	89		SW	1	Cloudy.
	54	2	0	54	55	30,14	88		SSW	1	Cloudy.
21	44	7	0	44	54	30,19	93	0,200	E	1	Cloudy.
	58	2	0	58	59	30,16	84		E	1	Hazy.
22	42	7	0	42	56	30,13	94		E	1	Foggy.
	57	2	0	56	59	30,07	76		E	1	Fine.
23	44	7	0	46	57	30,08	86		E	1	Cloudy.
	61	2	0	60	60	30,12	73		S	1	Fair.
24	42	7	0	42	58	30,22	87		NE	1	Fair.
	60	2	0	60	60	30,18	75		E	1	Hazy.
25	44	7	0	45	58	30,04	84		ENE	1	Hazy.
	61	2	0	60	61	30,04	73		ESE	1	Hazy.
26	46	7	0	46	60	30,06	81		NE	1	Hazy.
	65	2	0	65	62	30,08	71		E	1	Hazy.
27	45	7	0	45	60	30,18	83		NNE	1	Fine.
	63	2	0	63	62	30,16	68		E	1	Fine.
28	42	7	0	42	60	30,08	81		E	1	Fine.
	60	2	0	59	62	30,05	68		NE	1	Hazy.
29	46	7	0	46	61	29,98	85		NE	1	Hazy.
	62	2	0	60	62	29,90	67		W	1	Hazy.
30	45	7	0	46	60	30,03	81		W	1	Hazy.
	56	2	0	56	63	30,03	68		NW	1	Fine.
31	44	7	0	44	60	30,10	80		W	1	Fair.
	66	2	0	66	64	30,12	65		SW	1	Fine.

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for April, 1803.

1803	Six's Therm. least and greatest Heat.	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Hygrometer.	Rain. Inches.	Winds. Points.	Winds. Str.	Weather.
	°						°				
April 1	43	7	0	45	61	30,05	77		SSW	1	Fine.
	65	2	0	65	63	29,91	67		S	1	Fine.
2	51	7	0	51	63	29,65	76		S	1	Cloudy.
	61	2	0	61	64	29,57	72		S	2	Fair.
3	49	7	0	50	62	29,56	81	0,050	SE	1	Cloudy.
	57	2	0	57	63	29,58	77		S	2	Cloudy.
4	46	7	0	46	61	29,72	86	0,143	SW	1	Cloudy.
	56	2	0	55	63	29,82	68		SW	2	Fair.
5	44	7	0	46	61	30,04	83		SSW	2	Fine.
	57	2	0	56	62	30,08	70		SSW	2	Cloudy.
6	43	7	0	45	60	30,16	81		WSW	1	Cloudy.
	60	2	0	60	62	30,02	67		SSE	1	Fine.
7	46	7	0	48	60	29,68	84		E	1	Cloudy.
	59	2	0	59	62	29,62	62		E	2	Fine.
8	48	7	0	48	60	29,68	87	0,065	E	1	Rain.
	52	2	0	50	61	29,75	86		W	1	Rain.
9	46	7	0	46	60	29,87	90	0,310	N	1	Rain.
	57	2	0	54	60	29,91	85		E	1	Cloudy.
10	47	7	0	50	60	30,11	88	0,040	SW	1	Cloudy.
	58	2	0	58	61	30,18	76		NW	1	Hazy.
11	46	7	0	48	60	30,37	77		NE	1	Fair.
	62	2	0	62	63	30,39	65		NE	1	Hazy.
12	42	7	0	44	60	30,50	83		NE	2	Hazy.
	57	2	0	57	62	30,47	67		E	2	Fine.
13	39	7	0	44	60	30,44	83		NE	2	Hazy.
	60	2	0	60	65	30,44	72		E	2	Fine.
14	42	7	0	45	60	30,42	81		NE	1	Hazy.
	68	2	0	68	63	30,37	61		E	1	Fine.
15	44	7	0	48	61	30,29	77		NE	1	Fine.
	67	2	0	67	64	30,23	67		E	1	Fine.
16	45	7	0	49	62	30,05	78		E	1	Hazy.
	72	2	0	70	64	29,92	61		SE	2	Fine.

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for April, 1803.

1803	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H.	M.	°	°	Inches.		Inches.	Points.	Str.	
	°						°				
Apr. 17	51	7	0	53	62	29,91	83		S	2	Cloudy.
	61	2	0	60	64	29,96	64		SW	2	Fair.
18	47	7	0	47	62	29,77	83	0,035	WSW	1	Rain.
	54	2	0	54	62	29,91	67		WNW	2	Fair.
19	46	7	0	47	60	29,62	87		SSW	2	Rain.
	55	2	0	55	62	29,59	70		W	2	Cloudy.
20	42	7	0	44	58	29,60	74		W	2	Cloudy.
	53	2	0	51	61	29,60	68		WNW	2	Cloudy.
21	46	7	0	52	59	29,18	87	0,068	SW	2	Cloudy.
	55	2	0	54	60	29,19	71		WNW	2	Fair.
22	41	7	0	44	58	29,31	79		SW	1	Fine.
	56	2	0	51	60	29,36	73		W	2	Cloudy.
23	40	7	0	43	57	29,56	83	0,073	SSW	2	Fair.
	52	2	0	50	59	29,58	77		WSW	1	Rain.
24	38	7	0	41	57	29,66	85	0,032	SW	1	Fine.
	56	2	0	53	59	29,70	69		SW	1	Fair.
25	38	7	0	40	57	29,93	86	0,262	W	1	Hazy.
	53	2	0	51	58	29,99	72		W	1	Cloudy.
26	36	7	0	39	56	30,01	82		S	1	Fair.
	54	2	0	54	59	29,85	67		WSW	2	Cloudy.
27	43	7	0	45	56	29,61	81		N	1	Cloudy.
	51	2	0	50	58	29,61	74		NW	1	Fair.
28	36	7	0	39	56	29,90	82	0,016	NNE	2	Fine.
	47	2	0	45	57	29,97	72		N	2	Cloudy.
29	38	7	0	41	56	30,09	78		NE	1	Cloudy.
	50	2	0	50	57	30,09	73		NW	1	Cloudy.
30	43	7	0	44	56	29,94	79		SW	1	Cloudy.
	54	2	0	53	56	29,85	71		SW	1	Cloudy.

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for May, 1803.

1803	Six's Therm. least and greatest Heat.	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Hygrometer.	Rain. Inches.	Winds. Points.	Winds. Str.	Weather.
May 1	46°	7	0	46	55	29,60	88°	0,014	SW	1	Cloudy.
	57	2	0	57	57	29,56	82		SW	1	Cloudy.
2	50	7	0	51	56	29,37	88	0,083	SSW	2	Rain. [Much wind last night.
	54	2	0	53	58	29,34	86		SSW	2	Rain.
3	40	7	0	44	56	29,55	81	0,230	SW	2	Fine.
	54	2	0	52	58	29,51	79		SSW	2	Rain.
4	42	7	0	44	56	29,53	84	0,058	SSW	1	Fair.
	56	2	0	48	58	29,53	81		WSW	1	Rain.
5	42	7	0	44	56	29,79	86	0,157	SSW	1	Fair.
	59	2	0	58	58	29,85	70		SW	1	Cloudy.
6	46	7	0	47	57	30,03	83		W	1	Fair.
	61	2	0	60	59	30,04	69		N	1	Fair.
7	45	7	0	48	57	30,04	79		NE	1	Fair.
	64	2	0	64	60	30,02	66		NNE	1	Fine.
8	43	7	0	49	58	30,16	81		NE	1	Fine.
	67	2	0	66	60	30,16	66		NE	1	Fine.
9	49	7	0	52	60	30,14	72		NE	2	Fine.
	59	2	0	57	60	30,15	67		NE	2	Cloudy.
10	49	7	0	52	60	30,00	79		NW	1	Cloudy.
	60	2	0	60	61	29,96	69		NNE	1	Cloudy.
11	48	7	0	51	60	29,95	74		NW	1	Cloudy.
	58	2	0	58	61	29,98	66		NW	1	Fair.
12	50	7	0	53	60	29,80	82		W	1	Cloudy.
	62	2	0	62	62	29,78	66		W	2	Fair.
13	42	7	0	44	60	30,04	75		NW	2	Fine.
	58	2	0	58	62	30,13	64		NW	2	Fair.
14	42	7	0	45	60	30,25	76		SW	1	Fair.
	64	2	0	63	61	30,20	64		WNW	1	Fine.
15	46	7	0	48	60	30,20	73		WNW	2	Fine.
	57	2	0	55	60	30,21	64		NNW	2	Cloudy.
16	40	7	0	43	59	30,35	73		N	1	Fine.
	58	2	0	58	60	30,25	64		NW	1	Fine.

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1803	Six's Therm least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hy-gro-me-ter.	Rain.	Winds.		Weather.
		H.	M.	°	°	Inches.		Inches.	Points.	Str.	
May 17	48°	7	0	50	59	30,10	71°		N	2	Cloudy.
	57	2	0	57	60	30,11	65		N	2	Cloudy.
18	45	7	0	47	59	30,28	71		N	2	Fine.
	53	2	0	53	59	30,32	68		N	2	Cloudy.
19	38	7	0	44	58	30,28	75		NNE	2	Cloudy.
	53	2	0	53	59	30.26	74		NE	2	Cloudy.
20	44	7	0	44	59	30,18	79		NNE	1	Rain.
	49	2	0	47	59	30.10	88		NE	1	Rain.
21	47	7	0	50	59	30,18	88	0,270	NE	2	Cloudy.
	58	2	0	57	61	30,18	76		NE	1	Cloudy.
22	43	7	0	46	58	30,09	82		NE	2	Cloudy.
	54	2	0	52	60	30,04	74		NE	2	Cloudy.
23	45	7	0	48	58	29,98	83		NE	2	Cloudy.
	54	2	0	54	60	29,95	78		NE	2	Cloudy.
24	47	7	0	48	58	29,94	78		NNW	1	Cloudy.
	57	2	0	56	60	29,94	66		N	1	Cloudy.
25	48	7	0	51	58	29 87	80		WSW	2	Cloudy.
	60	2	0	59	60	29,80	78		SSW	2	Cloudy.
26	51	7	0	52	59	29 67	92	0,245	NW	1	Cloudy.
	67	2	0	66	61	29,78	62		SW	1	Hazy.
27	55	7	0	57	60	29,78	88		SW	2	Cloudy.
	67	2	0	67	62	29,84	72		WSW	2	Cloudy.
28	55	7	0	56	61	29,86	92	0,210	SSW	2	Cloudy.
	69	2	0	68	62	29,86	76		SW	1	Cloudy.
29	53	7	0	56	61	29,86	83		SW	2	Cloudy.
	61	2	0	61	62	29,78	78		SSW	2	Cloudy.
30	55	7	0	57	61	29 77	85	0,065	SW	2	Cloudy.
	66	2	0	66	62	29 77	73		SW	2	Fair.
31	53	7	0	55	61	29,97	88	0,353	WSW	2	Cloudy.
	67	2	0	67	62	30,01	69		SSW	2	Fair.

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1803	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hygro-meter.	Rain.	Winds.		Weather.
	°	H.	M.	°	°	Inches.	°	Inches.	Points.	Str.	
June 1	49	7	0	52	61	30,16	87	0,095	W	2	Fine.
	67	2	0	67	62	30,16	70		SW	2	Fair.
2	54	7	0	56	61	29,84	77		ESE	2	Cloudy.
	61	2	0	60	62	29,72	85		S	2	Rain.
3	50	7	0	53	61	29,74	83	0,285	S	2	Fair.
	62	2	0	60	62	29,69	79		S	2	Cloudy.
4	52	7	0	54	61	29,68	83	0,123	SSE	2	Fair.
	65	2	0	65	62	29,63	78		S	2	Cloudy.
5	53	7	0	55	62	29,63	83		E	1	Cloudy.
	61	2	0	61	62	29,64	80		N	1	Rain.
6	51	7	0	53	61	29,68	84	0,200	SW	1	Fair.
	65	2	0	65	62	29,70	74		SSW	1	Fair. Lightning with thund.
7	51	7	0	53	61	29,85	83	0,320	WSW	2	Cloudy.
	61	2	0	59	61	29,90	70		WSW	2	Cloudy.
8	50	7	0	50	61	29,80	84	0,040	E	1	Rain.
	54	2	0	54	61	29,71	91		NE	1	Rain.
9	50	7	0	53	60	29,87	90	0,318	E	1	Cloudy.
	61	2	0	60	61	29,75	86		E	1	Rain. Lightning with thunder.
10	52	7	0	53	60	29,81	80	0,661	SW	2	Fair.
	63	2	0	59	61	29,94	73		SW	2	Rain.
11	49	7	0	53	60	30,02	83	0,118	SSW	2	Cloudy.
	64	2	0	63	61	30,02	70		SSW	1	Cloudy.
12	57	7	0	58	60	30,00	90		S	1	Cloudy.
	66	2	0	66	61	29,96	82		S	1	Cloudy.
13	58	7	0	59	61	29,91	87	0,230	SW	1	Cloudy.
	67	2	0	66	62	29,89	77		SW	1	Cloudy.
14	58	7	0	58	61	30,02	75		WSW	1	Fair.
	73	2	0	73	64	30,04	67		SW	1	Fair.
15	53	7	0	57	62	30,04	79		SW	1	Fair.
	66	2	0	63	63	29,99	80		SW	2	Rain.
16	56	7	0	58	62	30,18	83	0,016	WSW	1	Cloudy.
	74	2	0	73	64	30,20	67		WSW	1	Fair.

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1803	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H.	M.	°	°	Inches.		Inches.	Points.	Str.	
June 17	58°	7	0	59	63	30,14	78°		W	2	Cloudy.
	71	2	0	71	65	30,17	68		W	2	Fair.
18	58	7	0	60	64	29,95	92	0,280	W	1	Rain.
	69	2	0	67	65	29,95	80		W	1	Cloudy.
19	57	7	0	57	64	29,95	85	0,097	WSW	1	Rain.
	61	2	0	60	64	29,84	84		S	1	Rain.
20	54	7	0	56	63	29,86	78	0,088	W	1	Cloudy.
	65	2	0	59	63	29,82	76		W	1	Rain.
21	50	7	0	52	62	29,93	82	0,460	NNW	2	Fine.
	64	2	0	64	64	30,12	61		NW	2	Fine.
22	49	7	0	55	62	30,19	77		WSW	1	Cloudy.
	59	2	0	58	62	30,08	78		SSW	1	Rain.
23	47	7	0	51	61	30,23	80	0,028	WNW	1	Fair.
	65	2	0	64	62	30,25	67		NW	1	Cloudy.
24	51	7	0	54	62	30,30	78		NNW	1	Fine.
	69	2	0	68	63	30,30	68		NNE	1	Hazy.
25	51	7	0	55	62	30,38	77		ENE	1	Hazy.
	68	2	0	67	63	30,38	69		E	1	Fair.
26	52	7	0	56	62	30,46	78		ENE	1	Cloudy.
	68	2	0	68	63	30,45	66		NE	1	Cloudy.
27	51	7	0	55	62	30,41	78		ENE	1	Cloudy.
	68	2	0	67	63	30,34	70		NE	1	Cloudy.
28	53	7	0	54	62	30,28	85		NE	1	Cloudy.
	70	2	0	70	64	30,23	71		E	1	Fine.
29	53	7	0	56	63	30,23	83		E	1	Cloudy.
	71	2	0	71	65	30,21	73		NE	1	Fair.
30	51	7	0	56	64	30,22	81		NE	1	Cloudy.
	67	2	0	67	65	30,23	70		NE	1	Fair.

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		H.	M.	°	°	Inches.		Inches.	Points.	Str.	
	°						°				
July 1	52	7	0	54	64	30,18	84		ENE	1	Cloudy.
	76	2	0	74.	66	30,13	71		NE	1	Fine.
2	59	7	0	63	65	30,00	81		NE	1	Fine.
	86	2	0	85	68	29,96	64		ESE	1	Hazy.
3	65	7	0	67	67	29,99	72	0,130	N	1	Fair.
	80	2	0	79	70	29,99	63		NW	1	Fine.
4	62	7	0	64	69	29,99	79		SW	1	Fine.
	81	2	0	80	70	29,96	62		SSW	2	Fine.
5	61	7	0	61	69	29.99	81		NW	1	Cloudy.
	71	2	0	70	69	29.97	68		SW	1	Cloudy.
6	52	7	0	57	68	29,97	75	0,030	NW	1	Fair.
	66	2	0	65	68	30,02	68		NW	1	Fair.
7	51	7	0	55	67	30,13	75	0,060	SW	2	Cloudy.
	68	2	0	67	67	30,07	69		S	2	Cloudy.
8	60	7	0	61	66	30,01	78	0,022	SSE	1	Fair.
	76	2	0	75	68	29,96	62		SSE	1	Fine.
9	59	7	0	62	68	29,98	84	0,135	WNW	1	Cloudy.
	75	2	0	73	68	30,05	62		WNW	1	Fine.
10	55	7	0	59	68	30,35	71		NW	1	Fine.
	73	2	0	71	68	30 38	62		N	1	Fine.
11	58	7	0	61	68	30,45	72		E	1	Fine.
	75	2	0	75	69	30,40	64		E	1	Fine.
12	57	7	0	62	69	30,28	73		E	1	Fair.
	79	2	0	79	70	30,18	66		ESE	1	Cloudy.
13	63	7	0	66	68	30,13	72		N	1	Fair.
	71	2	0	70	71	30,17	64		NW	1	Fine.
14	57	7	0	61	70	30,23	74		NE	2	Fine.
	65	2	0	63	67	30,27	68		ENE	2	Cloudy.
15	56	7	0	56	68	30,31	78		NE	2	Cloudy.
	64	2	0	64	68	30,28	70		NE	2	Cloudy.
16	56	7	0	57	67	30,22	77		NE	2	Cloudy.
	69	2	0	69	68	30,22	71		NE	2	Fair.

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		H.	M.	°	°	Inches.		Inches.	Points.	Str.	
July 17	° 55	7	0	56	66	30,22	° 81		NE	1	Cloudy.
	75	2	0	71	68	30,20	74		NE	1	Fair.
18	58	7	0	58	67	30,22	81		ENE	1	Cloudy.
	77	2	0	77	70	30,18	66		E	2	Fine.
19	59	7	0	64	68	30,14	76		E	2	Fair.
	79	2	0	79	71	30,08	67		E	2	Fine.
20	61	7	0	64	69	30,02	76		E	1	Fair.
	80	2	0	80	73	29,97	65		E	1	Fine.
21	56	7	0	62	70	30,07	91	0,395	NNE	1	Cloudy.
	71	2	0	71	70	30,14	75		NE	1	Fair.
22	58	7	0	60	69	30,29	83		E	1	Cloudy.
	78	2	0	76	70	30,28	64		SE	1	Fine.
23	59	7	0	63	70	30,28	77		S	1	Fair.
	82	2	0	80	71	30,21	63		WSW	1	Fine.
24	59	7	0	60	70	30,18	83		NE	1	Rain.
	71	2	0	70	71	30,17	67		NE	1	Cloudy.
25	51	7	0	58	69	30,19	77	0,055	NE	1	Fine.
	68	2	0	66	69	30,17	63		NE	1	Fair.
26	54	7	0	56	68	30,15	74		ENE	1	Fine.
	76	2	0	75	69	30,14	62		NW	1	Fair.
27	57	7	0	61	69	30,14	76		SW	1	Fair.
	75	2	0	74	69	30,10	65		E	1	Fair.
28	55	7	0	58	69	30,04	80		NE	1	Fair.
	76	2	0	76	70	29,97	67		E	1	Fine.
29	59	7	0	64	69	29,93	77		ENE	1	Hazy.
	75	2	0	75	69	30,01	85		S	1	Rain.
30	65	7	0	67	70	30,07	91	0,263	S	1	Cloudy.
	76	2	0	74	70	30,09	78		S	1	Fair.
31	66	7	0	67	70	30,06	84	0,278	S	1	Fine.
	81	2	0	81	73	29,98	67		S	1	Fine,

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1803	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hy-gro-me-ter.	Rain.	Winds.		Weather.
		H.	M.	°	°	Inches.		Inches.	Points.	Str.	
Aug. 1	° 64	7	0	65	72	29,92	° 80		SW	1	Fine.
	77	2	0	77	72	29,88	64		SSW	2	Fair.
2	60	7	0	62	69	29,95	81		SSW	2	Fair.
	73	2	0	73	70	29,89	67		SSW	2	Fair.
3	60	7	0	61	70	29,83	78		SSW	2	Fair.
	72	2	0	72	70	29,81	65		SSW	2	Fair.
4	53	7	0	56	68	30,10	76		SW	1	Fair.
	72	2	0	72	70	30,14	60		SW	1	Fine.
5	57	7	0	59	68	30,16	76		S	1	Fine.
	78	2	0	78	71	30,12	58		SSW	1	Fine.
6	62	7	0	63	68	29,99	83		SSW	2	Cloudy.
	76	2	0	75	71	29,89	69		SSW	2	Cloudy.
7	56	7	0	58	68	29,98	74		W	1	Fair.
	71	2	0	71	68	29,98	63		WNW	1	Cloudy.
8	56	7	0	57	67	30,04	74		NE	1	Fine.
	74	2	0	71	68	30,01	67		S	1	Fair.
9	58	7	0	60	67	29,98	76		S	1	Cloudy.
	75	2	0	74	69	29,92	68		S	1	Cloudy.
10	62	7	0	62	68	29,78	81	0,033	SSW	1	Cloudy.
	73	2	0	72	69	29,74	62		W	1	Fair.
11	55	7	0	59	68	29,92	80		WSW	1	Fair.
	70	2	0	70	68	29,98	67		WNW	1	Cloudy.
12	62	7	0	63	68	30,05	84		W	1	Cloudy.
	78	2	0	77	69	30,05	67		WNW	1	Fair.
13	60	7	0	62	68	30,09	82		SW	1	Fair.
	78	2	0	78	69	30,09	64		W	1	Fair.
14	62	7	0	63	68	30,19	81		NE	1	Fine.
	77	2	0	77	71	30,19	65		E	1	Fine.
15	61	7	0	63	69	30,20	83		E	1	Cloudy.
	76	2	0	75	71	30,16	68		NE	1	Fine.
16	60	7	0	64	71	30,13	88		NE	1	Hazy.
	81	2	0	80	73	30,08	58		E	2	Fine.

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		H.	M.	°	°	Inches.		Inches.	Points.	Str.	
Aug. 17	63°	7	0	65	71	30,09	82°		NE	1	Fine.
	79	2	0	78	74	30,09	63		E	1	Fine.
18	61	7	0	64	71	30,09	80		NE	1	Hazy.
	79	2	0	79	74	30,04	61		NE	1	Fine.
19	60	7	0	60	70	29,98	69		NW	2	Cloudy.
	68	2	0	68	72	29,99	65		NW	1	Cloudy.
20	52	7	0	53	68	30,01	73		WNW	1	Cloudy.
	66	2	0	66	70	30,01	67		NNW	1	Cloudy.
21	49	7	0	51	65	30,11	70		NE	1	Fine.
	65	2	0	62	68	30,11	62		N	2	Fair.
22	52	7	0	54	65	30,13	69		NW	1	Fair.
	67	2	0	67	68	30,14	64		WNW	1	Fair.
23	50	7	0	52	63	30,18	67		WNW	1	Fine.
	71	2	0	71	68	30,13	63		WNW	1	Fair.
24	53	7	0	57	64	30,06	78		W	1	Cloudy.
	74	2	0	73	67	30,04	62		W	1	Fine.
25	54	7	0	56	67	30,03	82		SW	2	Fine.
	76	2	0	76	69	29,98	66		SW	2	Fine.
26	55	7	0	57	68	30,16	72		NE	2	Fair.
	66	2	0	66	69	30,21	63		NE	2	Fine.
27	47	7	0	52	67	30,31	73		NE	2	Fine.
	67	2	0	67	69	30,29	62		NE	1	Fine.
28	47	7	0	53	67	30,21	72		N	1	Fine.
	74	2	0	73	68	30,17	60		SW	1	Fine.
29	56	7	0	58	68	30,16	82		SSW	1	Fine.
	75	2	0	74	70	30,08	62		SSW	1	Fine.
30	56	7	0	58	66	30,00	83	0,080	SW	1	Rain.
	60	2	0	57	67	30,02	93		NNE	1	Rain.
31	49	7	0	51	65	30,17	84	0,675	NNE	1	Fine.
	64	2	0	64	67	30,21	62		N	1	Fair.

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1803	Six's Therm. least and greatest Heat.	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Hygrometer.	Rain. Inches.	Winds. Points.	Winds. Str.	Weather.
Sept. 1	48	7	0	49	63	30,28	80		NW	1	Cloudy.
	63	2	0	63	65	30,28	67		NW	1	Cloudy.
2	49	7	0	51	64	30,28	80		W	1	Cloudy.
	69	2	0	69	65	30,24	71		W	1	Fair.
3	55	7	0	57	65	30,24	88		W	1	Fair.
	73	2	0	73	67	30,17	68		NW	1	Cloudy.
4	49	7	0	51	61	30,34	73		NE	2	Fine.
	60	2	0	60	64	30,36	62		NE	2	Fair.
5	43	7	0	44	63	30,42	73		WNW	1	Fine.
	64	2	0	63	65	30,38	62		W	1	Fine.
6	48	7	0	49	64	30,38	80		W	1	Fine.
	68	2	0	68	66	30,38	62		WSW	1	Fine.
7	52	7	0	52	64	30,39	78		WSW	1	Fine.
	70	2	0	69	65	30,33	67		W	1	Hazy.
8	53	7	0	54	64	30,26	76		S	1	Fine.
	75	2	0	74	67	30,22	65		SSW	1	Fine.
9	52	7	0	52	64	30,22	78		SW	1	Fine.
	73	2	0	72	67	30,22	67		NW	1	Fine.
10	50	7	0	51	65	30,33	75		WSW	1	Fine.
	66	2	0	65	66	30,31	68		W	1	Fine.
11	52	7	0	53	65	30,10	78		WSW	1	Fine.
	72	2	0	72	67	30,10	67		N	1	Fair.
12	45	7	0	46	65	30,33	73		NE	1	Fine.
	59	2	0	57	65	30,35	64		NE	2	Cloudy.
13	42	7	0	43	63	30,38	75		NE	2	Fine.
	60	2	0	59	63	30,34	64		NE	2	Fair.
14	45	7	0	46	62	30,32	72		NE	1	Fair.
	61	2	0	61	64	30,25	67		E	1	Fine.
15	45	7	0	46	62	30,12	78		NE	1	Fine.
	65	2	0	65	64	30,04	63		E	1	Fair.
16	46	7	0	47	62	29,92	73		SSW	1	Fine.
	66	2	0	65	64	29,86	64		WSW	1	Fine.

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	°	H.	M.	°	°	Inches.	°	Inches.	Points.	Str.	
Sep. 17	52	7	0	53	62	29,66	80	0,052	W	1	Fine.
	60	2	0	59	63	29,71	67		WNW	1	Cloudy.
18	45	7	0	47	61	29,78	73		SW	1	Cloudy.
	61	2	0	61	62	29,76	63		S	1	Fair.
19	48	7	0	50	60	29,85	79		SSW	1	Cloudy.
	60	2	0	60	61	29,83	84		S	1	Cloudy.
20	53	7	0	54	60	29,60	85	0,235	SE	1	Fine.
	58	2	0	55	61	29,38	90		S	1	Rain.
21	45	7	0	47	59	29,25	85	0,632	SW	2	Rain.
	58	2	0	57	60	29,62	73		NW	2	Cloudy.
22	40	7	0	42	58	29,97	80		W	1	Fine.
	56	2	0	56	58	30,04	69		NW	1	Cloudy.
23	44	7	0	46	58	30,09	79		W	1	Cloudy.
	56	2	0	56	58	30,11	69		NW	1	Cloudy.
24	38	7	0	40	57	30,27	79		W	1	Fine.
	56	2	0	56	58	30,32	72		NE	1	Fair.
25	39	7	0	41	56	30,37	81		NE	1	Fair.
	62	2	0	62	59	30,37	73		S	1	Fine.
26	43	7	0	43	57	30,39	83		SSW	1	Fair.
	62	2	0	62	61	30,36	71		SSE	1	Fine.
27	43	7	0	45	60	30,32	84		E	1	Fair.
	63	2	0	63	64	30,26	73		E	1	Fine.
28	48	7	0	48	60	30,20	92		NE	1	Fine.
	62	2	0	62	62	30,11	68		E	1	Fine.
29	46	7	0	46	60	29,94	87		NE	1	Fine.
	66	2	0	66	64	29,84	70		E	1	Fine.
30	48	7	0	48	61	29,75	89		E	1	Cloudy.
	59	2	0	59	62	29,70	86		E	1	Rain.

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	°	H.	M.	°	°	Inches.	°	Inches.	Points.	Str.	
Oct. 1	44	7	0	44	61	29,87	82	0,063	N	1	Fine.
	56	2	0	56	63	29,91	67		NNW	1	Fine.
2	40	7	0	41	60	29,97	83		WNW	1	Fine.
	54	2	0	54	61	29,96	63		NW	1	Cloudy.
3	42	7	0	43	59	29,78	85		WSW	1	Cloudy.
	50	2	0	50	60	29,73	73		E	1	Cloudy.
4	38	7	0	39	58	29,77	84		WSW	1	Cloudy.
	52	2	0	50	60	29,82	81		WSW	1	Rain.
5	38	7	0	39	58	29,97	87	0,115	W	1	Fine.
	55	2	0	54	60	30,02	72		WNW	1	Fair.
6	48	7	0	48	58	30,16	85		N	1	Cloudy.
	55	2	0	55	60	30,21	80		N	1	Cloudy.
7	45	7	0	45	59	30,20	80		NNE	1	Cloudy.
	54	2	0	54	61	30,05	69		WNW	1	Fine.
8	39	7	0	39	58	29,78	79	0,063	NNW	1	Fine.
	50	2	0	50	61	29,68	81		N	2	Fair.
9	46	7	0	46	58	29,86	83	0,060	NE	2	Cloudy.
	51	2	0	51	60	29,80	76		NE	2	Fair.
10	45	7	0	45	57	29,82	78		NNE	2	Fine.
	53	2	0	53	59	29,88	72		NNE	2	Cloudy.
11	42	7	0	43	57	30,05	80		WNW	1	Cloudy.
	53	2	0	53	59	30,10	70		NW	1	Cloudy.
12	48	7	0	49	57	30,11	80		W	1	Cloudy.
	57	2	0	57	59	30,14	73		WSW	1	Cloudy.
13	50	7	0	50	58	30,12	80		WSW	1	Cloudy.
	59	2	0	59	60	30,06	73		SSW	1	Cloudy.
14	47	7	0	48	58	29,83	78		SSE	1	Fair.
	55	2	0	53	59	29,72	81		SE	1	Rain.
15	43	7	0	43	58	29,73	90	0,173	WSW	1	Cloudy.
	60	2	0	60	61	29.72	74		SW	1	Cloudy.
16	50	7	0	51	59	29,81	91		SSW	1	Cloudy.
	63	2	0	63	62	29,80	75		SSW	1	Fair.

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	°			°	°	Inches.	°	Inches.			
Oct. 17	49	7	0	49	60	29,82	80		W	1	Fine.
	59	2	0	58	63	29,91	65		WNW	1	Fine.
18	51	7	0	55	60	29,96	87		WSW	2	Cloudy.
	65	2	0	65	62	30,04	79		SW	1	Cloudy.
19	53	7	0	56	62	30,20	86		SW	1	Cloudy.
	64	2	0	64	64	30,21	74		SW	1	Fine.
20	55	7	0	55	62	30,13	82		SW	2	Cloudy.
	64	2	0	64	63	30,18	78		WNW	1	Cloudy.
21	58	7	0	58	63	30,25	88		WSW	1	Cloudy.
	67	2	0	67	64	30,26	77		NW	1	Cloudy.
22	58	7	0	58	64	30,27	84		W	1	Cloudy.
	62	2	0	60	65	30,30	73		N	1	Cloudy.
23	45	7	0	46	63	30,38	86		WNW	1	Cloudy.
	58	2	0	58	65	30,41	74		NW	1	Fair.
24	43	7	0	43	62	30,46	83		NE	1	Foggy.
	58	2	0	58	65	30,46	75		E	1	Fine.
25	42	7	0	46	62	30,45	82		E	1	Cloudy.
	55	2	0	55	63	30,44	81		E	1	Cloudy.
26	46	7	0	48	62	30,48	83		E	1	Cloudy.
	50	2	0	50	62	30,47	81		E	1	Cloudy.
27	49	7	0	49	61	30,39	80		E	1	Cloudy.
	56	2	0	56	64	30,34	76		E	1	Fine.
28	39	7	0	40	60	30,28	83		NE	1	Foggy.
	54	2	0	54	63	30,22	77		E	1	Fine.
29	43	7	0	43	59	30,09	90		NE	1	Foggy.
	51	2	0	48	61	30,07	86		NE	1	Foggy.
30	43	7	0	46	59	30,07	87		NE	1	Cloudy.
	51	2	0	51	60	30,04	80		NE	1	Cloudy.
31	45	7	0	46	58	30,05	86		NE	1	Cloudy.
	51	2	0	50	59	30,08	76		NE	2	Cloudy.

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1803	Six's Therm. least and greatest Heat.	Time. H.	Time. M.	Therm. without.	Therm. within.	Barom.	Hy-gro-me-ter.	Rain.	Winds. Points.	Winds. Str.	Weather.
	°			°	°	Inches.	°	Inches.			
Nov. 1	44	7	0	44	58	30,18	72		NE	2	Cloudy.
	47	2	0	46	59	30,20	70		E	2	Cloudy.
2	40	7	0	41	56	30,20	70		E	2	Cloudy.
	45	2	0	44	59	30,23	68		NE	2	Fair.
3	34	7	0	35	54	30,24	76		NE	2	Cloudy.
	44	2	0	44	58	30,27	70		NE	2	Fine.
4	32	7	0	34	54	30,11	77		NE	2	Fine.
	43	2	0	43	58	29,98	72		NE	2	Fine.
5	35	7	0	37	54	29,68	73		E	2	Cloudy.
	39	2	0	39	55	29,51	73		E	2	Cloudy.
6	36	7	0	39	52	29,24	86		NE	1	Cloudy.
	51	2	0	48	55	29,15	91		ESE	1	Rain.
7	44	7	0	45	53	29,42	90	0,048	S	1	Fair.
	55	2	0	54	55	29,44	81		SE	2	Fair.
8	49	7	0	50	54	29,31	91	0,112	ESE	1	Rain.
	57	2	0	56	58	29,35	83		S	1	Fair.
9	49	7	0	49	55	29,10	93	0,332	SE	1	Rain.
	51	2	0	51	56	28,98	92		SSE	2	Rain.
10	46	7	0	49	56	28,69	93	0,141	S	2	Rain. [Much wind last night.
	54	2	0	52	58	28,71	82		S	2	Cloudy.
11	44	7	0	45	57	28,75	91	0,230	S	2	Rain.
	49	2	0	49	58	28,67	90		S	2	Rain.
12	43	7	0	44	57	28,76	85	0,266	SSW	2	Rain.
	51	2	0	50	58	28,91	81		SW	1	Fair.
13	43	7	0	43	56	29,18	87		WSW	1	Fine.
	51	2	0	50	58	29,30	81		W	1	Fair.
14	42	7	0	42	56	29,56	87	0,030	WSW	1	Cloudy.
	47	2	0	47	58	29,58	81		SW	1	Fair.
15	32	7	0	34	55	29,70	87		NE	1	Cloudy.
	49	2	0	49	57	29,68	83		SSE	1	Cloudy.
16	43	7	0	44	55	29,40	92	0,148	NE	1	Cloudy.
	53	2	0	53	56	29,18	90		S	2	Cloudy.

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1803	Six's Therm. least and greatest Heat.	Time. H.	Time. M.	Therm. without.	Therm. within.	Barom.	Hy-gro-me-ter.	Rain.	Winds. Points.	Winds. Str.	Weather.
	°			°	°	Inches.	°	Inches.			
Nov. 17	40	7	0	40	55	29,20	90	0,070	SW	1	Cloudy.
	45	2	0	45	57	29,33	78		WNW	1	Fine.
18	35	7	0	35	55	29,28	88		W	1	Fine.
	43	2	0	43	57	29,39	85		SW	1	Fine.
19	37	7	0	38	54	29,25	89		NW	1	Cloudy.
	45	2	0	45	57	29,33	84		NE	1	Fair.
20	38	7	0	38	54	29,50	84		NE	1	Cloudy.
	46	2	0	46	56	29,44	86		NE	1	Cloudy.
21	42	7	0	43	53	29,07	94	0,275	NE	1	Rain.
	50	2	0	49	57	29,06	96		NW	1	Rain.
22	40	7	0	48	54	29,07	92	0,202	S	2	Rain. [Much wind last night.
	50	2	0	50	56	29,04	87		S	2	Rain.
23	41	7	0	42	54	29,15	89	0,405	WSW	2	Cloudy.
	50	2	0	49	56	29,34	81		W	2	Fair.
24	34	7	0	34	53	29,61	87		W	2	Fair.
	43	2	0	43	54	29,69	79		W	1	Cloudy.
25	35	7	0	35	52	29,58	84	0,042	WNW	2	Fair.
	41	2	0	41	53	29,68	80		WNW	2	Fair.
26	36	7	0	36	52	29,88	83		NW	2	Fine.
	45	2	0	45	54	29,98	81		WNW	2	Cloudy.
27	31	7	0	31	52	30,07	90		SW	1	Foggy.
	41	2	0	41	53	30,08	90		SW	1	Cloudy.
28	40	7	0	44	52	29,94	94		SW	2	Cloudy.
	51	2	0	51	54	29,88	90		SW	2	Cloudy.
29	47	7	0	47	52	29,68	95		SW	1	Cloudy.
	53	2	0	51	55	29,50	90		NW	1	Rain.
30	39	7	0	39	52	30,15	87	0,140	NE	1	Fair.
	42	2	0	41	56	30,33	86		NE	1	Fine.

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for December, 1803.

1803.	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hy-gro-me-ter.	Rain.	Winds.		Weather.
		H.	M.	°	°	Inches.		Inches.	Points.	Str.	
	°						°				
Dec. 1	35	8	0	36	53	30,53	78		NW	1	Cloudy.
	48	2	0	48	54	30,45	87		SW	1	Cloudy.
2	45	8	0	46	53	30,13	93	0,031	SW	1	Cloudy.
	51	2	0	51	54	30,00	86		SW	1	Cloudy.
3	46	8	0	46	53	29,72	83		WNW	1	Cloudy.
	47	2	0	47	56	29,56	75		NW	1	Fair.
4	34	8	0	34	53	29,51	83		N	1	Cloudy.
	38	2	0	37	55	29,51	79		NNE	1	Fair.
5	28	8	0	31	51	29,43	82		NE	1	Cloudy.
	35	2	0	35	53	29,42	84		NE	1	Cloudy.
6	31	8	0	32	49	29,50	94		NE	1	Snow.
	35	2	0	34	51	29,54	89		NE	1	Fair.
7	30	8	0	31	48	29,72	92		NE	1	Cloudy.
	32	2	0	32	51	29,78	92		NE	1	Cloudy.
8	29	8	0	29	48	30,00	93		NE	1	Cloudy.
	33	2	0	32	50	30,08	89		NW	1	Fair.
9	21	8	0	21	46	30,18	90		W	1	Fine.
	33	2	0	32	50	30,16	89		W	1	Fair.
10	25	8	0	30	46	29,93	92		S	1	Cloudy.
	39	2	0	37	47	29,70	92		SE	1	Rain.
11	36	8	0	38	46	29,48	95	0,160	S	1	Fair.
	43	2	0	43	48	29,44	93		S	1	Cloudy.
12	36	8	0	38	47	29,37	95				Foggy.
	41	2	0	41	49	29,40	94		WNW	1	Foggy.
13	33	8	0	36	46	29,58	93	0,025	WSW	1	Rain.
	40	2	0	40	48	29,56	93		SW	1	Cloudy.
14	36	8	0	37	47	29,57	95				Foggy.
	41	2	0	41	49	29,54	95		E	1	Foggy.
15	37	8	0	37	47	29,27	94	0,137	E	1	Rain.
	40	2	0	40	50	29,17	94		E	1	Rain.
16	40	8	0	43	49	29,15	96	0,165			Foggy.
	48	2	0	48	52	29,13	97		E	1	Cloudy.

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for December, 1803.

1803	Six's Therm least and greatest Heat.	Time. H. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Hygrometer.	Rain. Inches.	Winds. Points.	Winds. Str.	Weather.
Dec. 17	45°	8 0	45	52	29,38	98°		NE	1	Cloudy.
	46	2 0	45	53	29,45	98		E	1	Rain.
18	44	8 0	45	51	29,48	98	0,125	E	1	Cloudy.
	48	2 0	47	53	29,47	98		E	1	Cloudy.
19	45	8 0	47	52	29,47	98	0,147	E	1	Foggy.
	50	2 0	50	53	29,50	98		E	1	Rain.
20	47	8 0	47	52	29,67	99	0,170	E	1	Foggy.
	48	2 0	48	55	29,70	99		E	1	Foggy.
21	47	8 0	48	53	29,72	99	0,048	ESE	1	Cloudy.
	52	2 0	52	54	29,70	97		SE	1	Cloudy.
22	49	8 0	49	55	29,82	97	0,025	SE	1	Cloudy.
	51	2 0	51	57	29,82	96		S	1	Cloudy.
23	44	8 0	44	55	29,75	91	0,075	SW	1	Fine.
	50	2 0	50	58	29,91	85		W	1	Fair.
24	47	8 0	55	56	29,70	97	0,360	S	2	Rain.
	55	2 0	55	58	29,64	95		S	2	Cloudy.
25	50	8 0	53	56	29,59	93	0,020	S	2	Rain. [Much wind last night.
	54	2 0	52	58	29,30	86		S	2	Cloudy.
26	48	8 0	52	56	29,62	92	0,022	S	2	Rain. [Much wind last night.
	53	2 0	52	58	29,64	87		S	1	Fair.
27	47	8 0	49	57	29,61	73		S	2	Cloudy.
	53	2 0	53	58	29,35	87		SE	2	Rain.
28	47	8 0	47	56	28,98	93	0,355	ENE	1	Rain.
	49	2 0	48	58	29,16	93		W	1	Fair.
29	48	8 0	46	56	29,58	92	0,625	SSW	1	Cloudy.
	51	2 0	50	57	29,48	92		S	2	Rain.
30	49	8 0	53	56	29,32	96	0,260	S	2	Rain.
	55	2 0	55	58	29,29	96		S	2	Cloudy.
31	47	8 0	47	56	29,43	94	0,340	W	1	Cloudy.
	48	2 0	48	58	29,53	94		W	1	Rain.

1803.	Six's Therm. without.			Thermometer without.			Thermometer within.			Barometer.*			Hygrometer.			Rain.
	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	
	Deg.	Deg.	Deg.	Deg.	Deg.	Deg.	Deg.	Deg.	Deg.	Inches.	Inches.	Inches.	Deg.	Deg.	Deg.	Inches.
January	48	19	35,3	48	19	35,7	56	43	49,0	30,18	29,05	29,65	95	73	86,9	1,544
February	53	19	38,3	52	20	38,5	59	42	49,1	30,48	29,27	29,86	95	69	85,1	0,744
March	66	25	44,4	66	26	44,5	64	48	55,1	30,39	29,38	30,03	94	65	80,5	0,449
April	72	36	50,4	70	39	51,0	65	56	60,3	30,50	29,18	29,89	90	61	76,2	1,094
May	69	38	53,0	68	44	53,8	62	55	59,4	30,35	29,34	29,95	92	64	76,3	1,685
June	74	49	59,0	73	50	59,8	65	60	62,2	30.46	29,63	30,02	91	61	78,2	3,359
July	86	51	66,3	85	54	67,2	73	64	68,8	30,45	29,93	30,13	91	62	72,7	1,368
August	81	47	64,6	80	51	65,4	74	63	68,7	30,31	29,81	30,06	93	58	71,3	0,755
September	75	38	55,1	74	40	55,5	67	56	62,4	30,42	29,25	30,12	92	62	74,3	0,919
October	67	38	51,1	67	39	51,3	65	57	60,6	30,48	29,72	30,07	91	63	79,4	0,474
November	57	31	43,7	56	31	44,0	59	52	55,3	30,33	28,67	29,50	96	68	84,8	2,441
December	55	21	44,7	56	21	43,3	58	46	52,6	30,53	28,98	29,61	99	75	92,1	3,090
Whole year			50,5			50,9			58,6			29,91			79,8	17,922

* The quicksilver in the bason of the barometer, is 81 feet above the level of low water spring tides at Somerset-house.

PHILOSOPHICAL

TRANSACTIONS,

OF THE

ROYAL SOCIETY

OF

LONDON.

FOR THE YEAR MDCCCIV.

PART II.

LONDON,

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MDCCCIV.

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ERRATUM

In Part I. page 145, line 20, *for* 9199, *read* 9919.

The three Plates in the second Part are numbered VII, VIII, IX, instead of VI, VII, VIII.

PHILOSOPHICAL

TRANSACTIONS.

IX. *Analytical Experiments and Observations on Lac. By* Charles Hatchett, *Esq. F. R. S.*

Read April 12, 1804.

THE period is uncertain when the substance called Lac, so curious in its origin and so useful to many arts, was first introduced into Europe; and, although it probably was known to the ancients, yet the inaccuracy of their descriptions precludes this from being stated as a positive fact.

The natives of India have long employed it for various purposes, exclusive of those which cause it to be in request with Europeans; but many of the Indian processes are undoubtedly as yet unknown to us.

One of these, of a very useful nature, was some time since obligingly communicated to me by CHARLES WILKINS, Esq. F. R. S. and has been the cause of this inquiry into the nature and properties of lac.

Mr. WILKINS informed me, that the Hindûs dissolve shell lac in water, by the mere addition of a little borax; and the solution,

being then mixed with ivory-black, or lamp-black, is employed by them as an ink, which, when dry, is not easily acted upon by damp or water. Upon trial, I found the fact to be exactly as Mr. WILKINS had stated, and therefore made other experiments; but the results of these I shall at present omit, as they will occur with more propriety and perspicuity in the latter part of this Paper.

In respect to the natural history of lac, we are much indebted to Mr. KERR,* Mr. SAUNDERS,† and Dr. ROXBURGH;‡ from whose valuable communications to this Society, we learn many curious particulars concerning the formation of this substance, which, from their accounts, and from inspection, evidently appears to be the nidus or comb of the insect called coccus or chermes lacca, deposited on branches of certain species of mimosa and other plants.

Lac is distinguished into four kinds; of which, however, only three are commonly known in commerce, *viz.* stick lac, seed lac, and shell lac; the difference of these, with that of the fourth, called lump lac, is as follows.

1. Stick lac, is the substance or comb in its natural state, incrusting small branches or twigs.

2. Seed lac, is said to be only the above, which has been separated from the twigs, and reduced into small fragments; but I suspect it to have undergone some other process, as I have

* Natural History of the Insect which produces the Gum Lacca. By Mr. JAMES KERR, of Patna. Phil. Trans. for 1781, p. 374.

† Some Account of the vegetable and mineral Productions of Boutan and Thibet. By Mr. ROBERT SAUNDERS. Phil. Trans. for 1789, p. 107.

‡ *Chermes Lacca.* By WILLIAM ROXBURGH, M. D. Phil. Trans. for 1791, p. 228.

found the best specimens to be very considerably deprived of the colouring matter.*

3. Lump lac, is formed from seed lac, liquefied by fire, and formed into cakes. And,

4. Shell lac, according to Mr. KERR and Mr. SAUNDERS, is prepared from the cells, liquefied, strained, and formed into thin transparent laminæ, in the following manner.

"Separate the cells from the branches; break them into small "pieces; throw them into a tub of water, for one day; wash "off the red water; dry the cells, and with them fill a cylindri-"cal tube of cotton cloth, two feet long, and one or two inches "in diameter; tie both ends, and turn the bag above a charcoal "fire; as the lac liquefies, twist the bag, and, when a sufficient "quantity has transuded the pores of the cloth, lay it upon a "smooth junk of the plantain tree, and with a strip of the plan-"tain leaf draw it into a thin lamella; take it off while flexible, "for in a minute it will be hard and brittle."†

The degree of pressure on the plantain tree, regulates (according to Mr. SAUNDERS) the thickness of the shell; and the quality of the bag determines its fineness and transparency.

Assam furnishes the greatest quantity of the whole of the lac now in use.‡

Mr. KERR (speaking of stick lac) observes, that the best lac is of a deep red colour; for, if it is pale and pierced at the top,

* Mr. WILKINS informs me that the crude lac, as it is taken from the branches and twigs of the trees, is usually deprived of its colouring matter by boiling, having been previously reduced, by pounding, into small fragments. In Bengal, the silk dyers are the people who thus produce what we call the seed lac, which they do for the sake of the colour.

† Phil. Trans. 1781, p. 378.

‡ Phil. Trans. 1789, p. 109.

the value is diminished, because the insects have left their cells, and consequently these can be of no use as a dye or colour, but probably may be better for varnishes.

The seed lac which I have examined, contained but little of the colouring matter, and appeared (as I have already observed) to have undergone some process of purification; but, of all the varieties, shell lac contains the least of the tinging substance, as may well be expected, when the mode of preparing it is considered.

It is remarkable, that although lac has been known, and imported into Europe, during so long a time that the date cannot now be ascertained, yet it has but little attracted the attention of chemists.

The first chemist of eminence who mentions it, and the only one who has subjected it to any thing like a regular examination, is the younger GEOFFROY, whose Paper is published in the *Mém. de l'Acad. de Paris* for the year 1714.* In this Paper, Mr. GEOFFROY seems to have been chiefly induced to examine it on account of its tinging substance; but he nevertheless has not neglected the substance which constitutes the cells. This he considers to be a sort of wax, very distinct from the nature of gum or resin. But it is to be observed, that he formed this opinion, not so much upon the results of chemical experiments, as upon the cellular construction observed in the stick lac, which, as he justly remarks, demonstrates it to be formed by insects, after the manner that the honeycomb is formed by bees; and that it is not therefore, as some have supposed, a gum or

* Observations sur la Gomme Lacque, et sur les autres Matières animales qui fournissent la Teinture de Pourpre. Par M. GEOFFROY le jeune. *Mém. de l'Acad.* 1714, p. 121.

resin, which has exuded from vegetables simply punctured by insects.*

Geoffroy and Lemery obtained from lac, by distillation, some acid liquor, and a butyraceous substance. Moreover, Geoffroy observes, that when stick lac was thus treated, some ammonia was also obtained, but not when seed lac was employed.

He also mentions another sort of lac, brought from Madagascar, and called by the natives *Lit-in-bitsic.* This substance, he says, is scarcely to be distinguished from bees-wax, which it much resembles in colour and odour; and that it is produced by a grayish insect, much larger than the chermes lacca. It is evident however, from Geoffroy's description, that this substance is very different from the common lac; and there can be little doubt, but that it is the same as that which was, a few years ago, examined by Dr. Pearson, under the name of white lac, a substance resembling the Pé-la of the Chinese.†

Geoffroy (as I have stated) considered lac as a sort of wax; and since his time it has scarcely, if at all, been subjected to chemical examination; it is not therefore surprising, that the opinions of chemists concerning it have been various. Chaptal adopts the opinion of Geoffroy, and calls it a kind of wax;‡ but Gren § and Fourcroy || regard it as a true resin.

* Mr. Kerr observes, that as a red substance is obtained by incision from the plaso tree, very analogous to lac, it is probable, that the insects have little trouble in animalizing the sap of these trees, in the formation of their cells. Phil. Trans. 1781, p. 377.

† Phil. Trans. 1794, p. 383.

‡ Chaptal's Elements; English edition. Vol. III. p. 387.

§ Principles of modern Chemistry. Vol. I. p. 388.

|| *Systême des Connoissances chimiques.* Tome V. p. 624.

§ I.

EFFECTS OF DIFFERENT MENSTRUA ON THE VARIETIES OF LAC.

1. When water is poured on stick lac, which has been separated from the vegetable branches, and reduced to a coarse powder, it immediately begins to be tinged with red; and, with the assistance of heat, a deep coloured crimson solution is formed.

Repeated operations of this kind reduce stick lac to a yellowish-brown substance; and the water no longer receives any colour.

The portion thus separated from stick lac has, upon an average, amounted in my experiments to about 10 *per cent.* but this is not to be regarded as the total quantity, for a part is obstinately retained by the resin and other ingredients, so that it cannot be completely separated; and moreover, very considerable variations must be expected in different samples.

Fine seed lac did not afford more than $2\frac{1}{2}$ or 3 *per cent.* of the colouring substance; and shell lac, when treated in the same manner, (*i. e.* merely with water,) did not yield more than $\frac{1}{2}$ *per cent.*

2. Alcohol dissolves a considerable portion of each of the different kinds of lac; and, when heat is not employed, the dissolved part is resin, combined with some of the colouring matter; but, if the lac is digested with heated alcohol, then the solution is more or less turbid, in consequence of some of the other ingredients becoming mixed and suspended; so that it is afterwards extremely difficult to obtain it in a state of purity and transparency, either by repose or by filtration.

The resin may be obtained by immediately subjecting the

solution to evaporation or distillation, or by previously pouring it into water with which a small quantity of muriatic or acetic acid has been mixed; for thus, when the whole is heated, a curdy precipitate of resin is formed, and may be separated by a filter, after which, the liquor may be evaporated, in order to obtain any resin, or other substance, which may remain in solution after the first operation.

The solution formed by digesting stick lac in alcohol, without heat, is of a dark brownish-red colour, and the insoluble part subsides, in the state of a dark coloured magma; this retains the greater part of the colouring matter, which, as I have already observed, is most easily soluble in water.

The proportion of resin thus dissolved, when stick lac is treated with alcohol, has, in my experiments, amounted to 67 or 68 *per cent.* but this must depend on the quality of the samples.

The seed lac which I examined was very pure, and yielded to alcohol about 88 *per cent.* of resin: it contained but little of the colouring matter; and the other substances subsided, and formed a cloud at the bottom of the glass vessel.

Shell lac in small fragments, by simple digestion with alcohol, afforded in the first instance nearly 81 *per cent.* Part of the resin, however, still remained mixed with the residuum, and could not be separated but by subsequent operations: this part amounted to about 10; so that the total quantity of resin, in the shell lac which I employed, may be estimated at 91 *per cent.*

When the shell lac was only reduced into small fragments, these (after the separation of the first portion of resin) retained their figure, but were become more bulky, very elastic,

and nearly white. I at first therefore suspected, that some caoutchouc was present in lac; but, finding that boiling water destroyed this elasticity, I was induced to make subsequent experiments, by which I discovered, that the elasticity of this residuum, was principally owing to a substance which appeared to possess the properties of vegetable gluten. This, however, I shall more fully notice in another part of the Paper.

The resin obtained from the varieties of lac is brownish yellow, and is not so brittle as the generality of other resinous substances.

3. Sulphuric ether does not seem to act so powerfully upon the varieties of lac as alcohol; for, as a great part of the resin is protected by the colouring matter, and by the other ingredients which are insoluble in ether, it naturally follows, that less of it can be separated by this liquid than by alcohol.

The different kinds of lac which have been digested in ether are considerably softened, although in other respects very little alteration is produced. Ether, therefore, is not the best menstruum for lac; but, under certain circumstances, it may be occasionally employed with advantage, for the purpose of analysis.

4. Concentrated sulphuric acid acts in the first instance on the colouring matter of lac; but, after a short digestion in a sand-bath, the whole is converted into a reddish-brown thick liquor, which soon becomes black; and the chief part of the lac is separated, in an insoluble state, resembling coal.

During the solution of lac in sulphuric acid, a considerable quantity of sulphureous acid gas is evolved.

5. When lac is digested with nitric acid, nitrous gas is at first produced; the lac swells much, and is converted into

a deep yellow opaque brittle substance, which, by a sufficiency of nitric acid, and continuation of the digestion during about 48 hours, is dissolved.

The solution however is turbid, and, when poured into a large quantity of distilled water, deposits some yellowish flocculi, which, being collected, are found to be a sort of wax.

The filtrated liquor is of a bright golden yellow; and, when saturated by ammonia, changes to orange colour, but does not yield any precipitate, nor any traces of oxalic or malic acid.

This yellow nitric solution is converted, by evaporation, into a deep yellow substance, which burns like resin, but is soluble in boiling water.

The alkalis and lime, being added to this aqueous solution, do not produce any precipitate, but the yellow colour is very considerably deepened; and, by evaporation, an orange-coloured substance is obtained, which is still easily soluble in water, and consists of the deep yellow substance abovementioned, combined with the alkali or lime.

6. Muriatic acid dissolves the colouring matter and gluten of lac; but its action on these is feeble, unless the resin has been previously separated.

7. Acetous acid, in its effects, much resembles muriatic acid.

8. Stick lac, seed lac, and shell lac, are partially dissolved by acetic acid; and, if this be heated, a considerable portion is taken up.

The dissolved part consists of the colouring extract, of resin, and of gluten; the wax being the only ingredient which is insoluble in this menstruum; but a portion of the former substances,

being enveloped by the wax, are protected from the action of the acetic acid.

The acetic solution of lac becomes turbid when cold, and deposits part of the resin; a portion however remains in solution, and may be precipitated by water; after which, the liquor retains some gluten and colouring extract, which may be precipitated by saturating the acid with an alkali, and by subsequent boiling.

For the reasons above stated, it would be difficult to effect a complete solution of lac by means of acetic acid; but this may nevertheless be advantageously employed in analytical operations, when alternately used with alcohol.

9. A saturated solution of boracic acid in water, dissolves the colouring extract; but, as the effect does not surpass that of water alone, we may conclude that lac is little, if at all, acted upon by boracic acid.

10. It has been already stated, that sub-borate of soda or borax has a powerful effect on lac, so as to render it soluble in water; and, as the preceding experiments prove that boracic acid alone scarcely acts upon lac, there is every reason to believe, that the excess of soda present in borax is the active substance; and this conclusion will be confirmed, by the results of subsequent experiments made with the alkalis.

In order to render lac (especially shell lac) soluble in water, about $\frac{1}{5}$ of borax is necessary; and this may be previously dissolved in the water, or may be mixed and added together with the lac.

The best proportion of water to that of lac is 18 or 20 to 1. So that 20 grs. of borax, and four ounces of water, are, upon

an average, requisite to dissolve 100 grs. of shell lac; but more water may be occasionally added, to supply the loss caused by evaporation during the digestion, which should be made nearly in a boiling heat.

This solution of shell lac is turbid, and of a reddish-brown colour; when considerably diluted with water and agitated, a weak lather is formed; it is decomposed by acids, and the lac is precipitated in yellow flocculi, which do not apparently differ from the lac originally employed.

The general properties of the solution show, that it is a saponaceous compound, which, being used as a varnish or vehicle for colours, becomes (when dry) difficultly soluble in water, although this was the liquid employed to form the solution.

A white thick scum or cream collects on the surface of this liquid, after it has been suffered to remain tranquil for some time, and is found to be produced by a sort of wax, which I shall more particularly notice when the analyses of the varieties of lac are described; but, in the present case, this wax appeared in some degree to be converted into an almost insoluble soap by the alkali of the borax, and may be regarded as the principal cause of the turbidness of the solution.

11. The lixivia of pure soda and of carbonate of soda completely dissolve the different kinds of lac; and these solutions exactly resemble those formed by means of borax, excepting that they are deeper coloured.

Rather less than $\frac{1}{5}$ of carbonate of soda is required to dissolve shell lac; and this solution, when dried, is sooner affected by damp or water than the solution prepared by borax.

12. Lixivium of pure or caustic potash speedily dissolves the

varieties of lac, and forms saponaceous solutions, similar to that in which borax was employed, exclusive of the colour, which is deeper, and more approaching to purple.

Lixivium of carbonate of potash extracts a great part of the colouring matter, but does not form so complete a solution of the entire substance of lac, as when pure potash is employed.

The above alkaline solutions, by repose, afford the waxen soap which has been mentioned; and acids, being added to these solutions, and to that formed by borax, precipitate the lac in a flocculent state, and of a yellow or buff colour, which precipitate, when melted, becomes similar to the lac originally employed. If however an alkaline solution of shell lac (prepared, for instance, with soda) be gradually dropped into a sufficient quantity of muriatic acid diluted with an equal portion of water, and nearly heated to the boiling point, and if after boiling the whole for about one hour the coagulum be separated, and the clear liquor be carefully saturated with soda, and again made to boil, a small quantity of a flocculent precipitate is obtained, which was found to be analogous to precipitated vegetable gluten, combined with some of the colouring extract.

13. Pure ammonia, and carbonate of ammonia, readily act upon the colouring matter of lac, but do not completely dissolve the entire substance.

§ II.

ANALYTICAL EXPERIMENTS ON STICK, SEED, AND SHELL LAC.

Lac, when placed on a red-hot iron, at first contracts, and then melts, emitting a thick smoke, of a peculiar but rather pleasant odour; after which, a light spongy coal remains.

Distillation of Stick Lac.

100 grains of the best stick lac, separated as much as possible from the twigs, were put into a glass retort, to which a double tubulated receiver and hydro-pneumatic apparatus were adapted. Distillation was then gradually performed, with an open fire, until the bottom of the retort became red-hot.

The products thus obtained were,	Grs.
1. Water slightly acid - - -	10.
2. Thick brown butyraceous oil - -	59.
3. Spongy coal - - - -	13.50
4. A small portion of carbonate of ammonia, with a mixture of carbonic acid, carbonated hydrogen, and hydrogen gas, which may be estimated at	17.50
	100.

Seed Lac.

100 grains of very pure seed lac were distilled in a similar manner, and afforded,

1. Acidulated water - - - -	6.
2. Butyraceous oil - - - -	61.
3. Spongy coal - - - - -	7.
4. Mixed gas nearly as before, but without ammonia, amounting by estimation to - -	26.
	100.

Shell Lac.

100 grains of shell lac, treated as above, yielded,

1.	Acidulated water - - - -	6.
2.	Butyraceous oil - - - -	65.
3.	Spongy coal - - - -	7.50
4.	Mixed gas, amounting by estimation to -	21.50
		100.

The coal of the shell lac, by incineration, afforded about one grain of ashes, which contained a muriate, probably of soda, and a little iron, with some particles of sand, which may be regarded as extraneous.

Analysis of Stick Lac.

A. 200 grains of stick lac, picked and reduced to powder, were digested in a pint and a half of boiling distilled water during 12 hours. The liquor was transparent, and of a beautiful deep red; this was decanted into another vessel; and the operation was repeated, with fresh portions of water, until it ceased to be tinged; the lac then appeared of a pale yellowish-brown colour.

The whole of the aqueous solution being evaporated, left a deep red substance, which possessed the general properties of vegetable extract, and weighed 18 grains.

B. The dried lac was digested for 48 hours, without heat, in eighteen ounces of alcohol; and the clear tincture being cautiously decanted, different portions of alcohol were added, and the digestion was repeated, until the alcohol ceased to produce any effect.

The whole of the solutions in alcohol were then poured into distilled water, which was heated, and an attempt was made to separate the precipitated substance by filtration; but, as this did not succeed, on account of the filter speedily becoming clogged, the whole was subjected to gentle distillation; by which, a brownish-yellow resin was obtained, amounting in weight to 136 grains.

C. The remainder of the lac was again digested in boiling distilled water; by which, 2 grains of the colouring extract were obtained.

D. The residuum was then digested with one ounce of muriatic acid diluted with two ounces of water, which, by boiling, became of a bright pale red, but changed to purple, when saturated with a solution of carbonate of potash.

A flocculent precipitate was thus obtained, which possessed the characters of precipitated vegetable gluten combined with some of the colouring extract; this, when completely dried, weighed 11 grains.

E. There now remained 25 grains, which evidently consisted of a sort of wax, mixed with small parts of twigs and other extraneous substances.

A part of the wax was separated by heat and pressure in a piece of linen; and another portion was separated by digestion in olive oil, which assumed the consistency of an unguent.

The residuum was then boiled with lixivium of potash, and became tinged with purple, in consequence of some of the colouring extract which had not been dissolved by the preceding operations.

The undissolved part, now consisting only of the extraneous vegetable and other substances, weighed 13 grains; so that the

wax, with a small portion of the colouring extract, may be estimated at 12 grains.

By the above process, 200 grains of stick lac afforded,

			Grs.
A. C.	Colouring extract	18 2	20
B.	Resin		136
D.	Vegetable gluten		11
E.	Wax, with a little colouring extract, about		12
	Extraneous substances.		13
			192.

Analysis of Seed Lac.

200 grains of very pure seed lac were subjected to operations very similar to those which have been described, and afforded,

	Grs.
Colouring extract	5
Resin	177
Vegetable gluten	4
Wax	9
	195.

Analysis of Shell Lac.

A. 500 grains of this substance were first treated with boiling distilled water, as above-mentioned, and yielded of extract only 2.50 grains.

B. The 497.50 grains which remained, were then digested with different portions of cold alcohol, until this ceased to produce any effect; the resin which was thus separated, amounted to 403.50 grains.

C. As the shell lac had not been reduced into powder, but only into small fragments, these were become white and elastic,

and, when dry, were brittle, and of a pale brown colour; the whole then weighed 94 grains.

D. These 94 grains were digested in diluted muriatic acid; and the acid, being afterwards saturated with solution of carbonate of potash, afforded a flocculent precipitate, (resembling that obtained from solutions of vegetable gluten,) which, when dry, weighed 5 grains.

E. Alcohol acted but feebly on the residuum; it was therefore put into a matrass, with three ounces of acetic acid, and was suffered to digest without heat during six days, the vessel being at times gently shaken; the acid thus assumed a pale brown colour, and was very turbid. The whole was then added to half a pint of alcohol, and was digested in a sand-bath; by which a brownish tincture was formed, and at the same time a quantity of a whitish flocculent substance was deposited, which, being collected, well washed with alcohol on a filter, and dried, weighed 20 grains.

This substance was white, light, and flaky, and, when rubbed by the nail, it became glossy, like wax; it also easily melted, was absorbed by heated paper, and, when placed on a coal or hot iron, emitted a smoke, the odour of which very much resembled that of wax, or rather spermaceti.

F. The solution formed by acetic acid and alcohol, being filtrated, was poured into distilled water, which immediately became milky; and, being heated, the greater part of the resin which had been dissolved assumed a curdy form, and was partly separated by a filter, and partly by distilling off the liquor; this portion of resin amounted to 51 grains.

G. The filtrated liquor, from which this resin had been separated, was saturated with a solution of carbonate of potash;

and, being heated, a second precipitate of gluten was obtained, which, when well dried, weighed 9 grains.

The 500 grains of shell lac thus yielded,

		Grs.
A.	Extract	2.50
B. F.	Resin	454.50
D. G.	Vegetable gluten	14.
E.	Wax	20.
		491.

The mode of analysis adopted for the shell lac, must undoubtedly appear less simple than that which was employed for seed and stick lac; but, upon the whole, it was attended with advantages; for the shell lac being in small fragments, and not in the state of a powder, considerably facilitated the decantation of the solution in alcohol from the residuum; and although, in this last, a portion of the resin was protected from the action of the alcohol, by being enveloped in the gluten and wax, yet, by the assistance of acetic acid, the remainder of the resin, as well as the whole of the gluten, were dissolved; the wax was obtained in a pure state; and a separation of the resin from the gluten was afterwards easily effected, by the method which has been described. As therefore acetic acid is capable of dissolving resin, gluten, and many other of the vegetable principles, it certainly may be regarded as a very useful solvent, in the analysis of bodies appertaining to the vegetable kingdom.

From the results of the preceding analyses it appears, that the different kinds of lac consist of four substances, namely, extract, resin, gluten, and wax, the separate properties of which shall now be more fully considered.

Properties of the colouring Extract of Lac.

1. When dry, it is of a deep red colour, approaching to purplish crimson.

2. Being put on a red-hot iron, it emits much smoke, with a smell somewhat resembling burned animal matter, and leaves a very bulky and porous coal.

3. Water, when digested with it in a boiling heat, partially dissolves it; but the residuum was found to be absolutely insoluble in water.

4. Alcohol acts but slowly on it; and, in a digesting heat, dissolves less than water. The colour of the solution is also not so beautiful; and a considerable part of the residuum left by alcohol was, when digested with water, found to be soluble, although this was not the case, when the residuum left by water was eated with alcohol.

5. It is insoluble in sulphuric ether, excepting a very small portion of resin, which appeared to be accidentally mixed with it.

6. Sulphuric acid readily dissolves it, and forms a deep brownish-red solution, which, being diluted with water, and saturated with potash, soda, or ammonia, becomes changed to a deep reddish-purple.

7. Muriatic acid dissolves only a part: the solution is of the colour of port wine, and, by the alkalis, is changed to a deep reddish-purple.

8. Nitric acid speedily dissolves it: the solution is yellow, and rather turbid; but the red colour is not restored by the alkalis, for these only deepen the yellow colour. This nitric solution did not afford any trace of oxalic acid.

9. Acetic acid dissolves it with great ease, and forms a deep brownish-red solution.

10. Acetous acid does not dissolve it quite so readily, but the solution is of a brighter red. Both of the above, when saturated with alkalis, are changed to a deep reddish-purple.

11. The lixivia of potash, soda, and ammonia, act powerfully on this substance, and almost immediately form perfect solutions, of a beautiful deep purple colour.

12. Pure alumina, put into the aqueous solution, does not immediately produce any effect; but, upon the addition of a few drops of muriatic acid, the colouring matter speedily combines with the alumina, and a beautiful lake is formed.

13. Muriate of tin produces a fine crimson precipitate, when added to the aqueous solution.

14. A similar coloured precipitate is also formed, by the addition of solution of isinglass.

These properties of the colouring substance of lac, especially its partial solubility in water and in alcohol, and its insolubility in ether, together with the precipitates formed by alumina and muriate of tin, indicate that this substance is vegetable extract, perhaps slightly animalized by the coccus.

The effects which it produced on gelatin, also demonstrate the presence of tannin; but this very probably was afforded by the small portions of vegetable bodies, from which the stick lac can seldom be completely separated.

Properties of the Resin of Lac.

This substance is of a brownish-yellow colour; and, when put on a red-hot iron, it emits much smoke, with a peculiar sweet odour, and leaves a spongy coal.

It is completely soluble in alcohol, ether, acetic acid, nitric acid, and the lixivia of potash and soda.

Water precipitates it from alcohol, ether, acetic acid, and partially from nitric acid; and it possesses the other general characters of a true resin.

Properties of the Gluten of Lac.

It has been already observed, that when small pieces of shell lac have been repeatedly digested in cold alcohol, they become white, bulky, and elastic. By drying, these pieces become brownish and brittle; the elasticity is also destroyed by boiling water, exactly as when the gluten of wheat is thus treated.

If the pieces of shell lac, after the digestion in alcohol, be digested with diluted muriatic acid, or with acetic acid, the greater part of the gluten is dissolved, and may be precipitated, in a white flaky state, by alkalis; but, if these last be added to excess, and heat be applied, then the glutinous substance is re-dissolved, and may be precipitated by acids.

If the pieces of shell lac, after digestion in alcohol, be treated with alkaline lixivia, then the whole is dissolved, and forms a turbid solution. But, when acids are employed, the chief part of the gluten is alone acted upon, and a considerable residuum is left, consisting of the wax, some of the resin, and a portion of gluten, which has been protected from the action of the acid by the two former substances.

The above properties indicate a great resemblance between this substance and the gluten of wheat; I therefore have called it gluten, but, at a future time, I intend to subject it to a more accurate examination.

Properties of the Wax of Lac.

If shell lac be long and repeatedly digested in boiling nitric acid, the whole is dissolved, excepting the wax, which floats on the surface of the liquor, like oil, and, when cold, may be collected; or it may be more easily obtained in a pure state, by digesting the residuum left by alcohol in boiling nitric acid.

The wax thus obtained, when pure, is pale yellowish white, and (unlike bees wax) is devoid of tenacity, and is extremely brittle.

It melts at a much lower temperature than that of boiling water, burns with a bright flame, and emits an odour somewhat resembling that of spermaceti.

Water does not act upon it, neither does cold alcohol; but this last, when boiled, partially dissolves it, and, upon cooling, deposits the greater part; a small portion, however, remains in solution, and may be precipitated by water.

Sulphuric ether, when heated, also dissolves it; but, upon cooling, nearly the whole is deposited.

Lixivium of potash, when boiled with the wax, forms a milky solution; but the chief part of the wax floats on the surface, in the state of white flocculi, and appears to be converted into a soap of difficult solubility; it is no longer inflammable, and, with water, forms a turbid solution, from which, as well as from the solution in potash, the wax may be precipitated by acids.

Ammonia, when heated, also dissolves a small portion of the wax, and forms a solution very similar to the former.

Nitric and muriatic acids do not seem to act upon the wax; the effects of sulphuric acid have not been examined.

When the properties of this substance are compared with those of bees-wax, a difference will be perceived; and, on the contrary, the most striking analogy is evident, between the wax of lac and the myrtle wax which is obtained from the Myrica cerifera.

An account of the latter substance has been published by Dr. Bostock, of Liverpool, in Nicholson's Journal, with comparative Experiments and Observations on Bees-Wax, Spermaceti, Adipocire, and the crystalline Matter of biliary Calculi.*

The properties of the myrtle wax, as described in Dr. Bostock's valuable Paper, so perfectly coincide with those which I have observed in the wax of lac, that I cannot but consider them as almost the same substance; indeed I think they may be regarded as absolutely identical, if some allowance be made for the slight modifications which have been produced by the different mode of their formation.

From the preceding experiments and analyses we find, that the varieties of lac consist of the four substances which have been described, namely, extractive colouring matter, resin, gluten, and a peculiar kind of wax. Resin is the predominant substance; but this, as well as the other ingredients, is liable, in a certain degree, to variation in respect to quantity.

According to the analyses which have been described, one hundred parts of each variety of lac yielded as follows.

* Nicholson's Journal for March, 1803, p. 129.

Stick Lac.

Resin - - - -	68.
Colouring extract - -	10.
Wax - - - -	6.
Gluten - - -	5.50
Extraneous substances - -	6.50
	96.0.

Seed Lac.

Resin - - - -	88.50
Colouring extract - -	2.50
Wax - - - -	4.50
Gluten - - - -	2.
	97.50.

Shell Lac.

Resin - - -	90.90
Colouring extract - -	0.50
Wax - - - - -	4.
Gluten - - - -	2.80
	98.20.

The proportions of the substances which compose the varieties of lac, must however be subject to very considerable variations; and we ought therefore only to consider these analyses in a general point of view. Hence we should state, that lac consists principally of resin, mixed with certain proportions of a peculiar kind of wax, of gluten, and of colouring extract.

The relative quantity of the two latter ingredients, very considerably affect the characters of the lacs; for instance, we may

observe, that the glutinous substance, when present in shell lac in a more than usual proportion, probably produces the defect observed in some kinds of sealing wax, which, when heated and burned, become blackened by particles of coal; for the gluten affords much of this substance, and does not melt, like the resin and wax. From what has been stated, therefore, lac may be denominated a *cero-resin*, mixed with gluten and colouring extract.

§ III.

GENERAL REMARKS.

From the whole of the experiments which have been related, it appears, that although lac is indisputably the production of insects, yet it possesses few of the characters of animal substances; and that the greater part of its aggregate properties, as well as of its component ingredients, are such as more immediately appertain to vegetable bodies.

Lac, or gum lac, as it is popularly but improperly called, is certainly a very useful substance; and the natives of India furnish full proofs of this, by the many purposes to which they apply it.

According to Mr. Kerr, it is made by them into rings, beads, and other female ornaments.

When formed into sealing-wax, it is employed as a japan, and is likewise manufactured into different coloured varnishes.

The colouring part is formed into lakes for painters: a sort of Spanish wool for the ladies is also prepared with it; and, as a dying material, it is in very general use.

The resinous part is even employed to form grindstones, by

melting it, and mixing with it about three parts of sand. For making polishing grindstones, the sand is sifted through fine muslin; but those which are employed by the lapidaries, are formed with powder of corundum, called by them Corune.*

But, in addition to all the above uses to which it is applied in India, as well as to those which cause it to be in request in Europe, Mr. WILKINS's Hindû ink occupies a conspicuous place, not merely on account of its use as an ink, but because it teaches us to prepare an aqueous solution of lac, which probably will be found of very extensive utility.

This solution of lac in water may be advantageously employed as a sort of varnish, which is equal in durability, and other qualities, to those prepared with alcohol; whilst, by the saving of this liquid, it is infinitely cheaper.

I do not mean however to assert that it will answer equally well in all cases, but only that it may be employed in many. It will be found likewise of great use as a vehicle for colours; for, when dry, it is not easily affected by damp, or even by water.

With a solution of this kind, I have mixed various colours, such as, vermillion, fine lake, indigo, Prussian blue, sap green, and gamboge; and it is remarkable, that although the two last are of a gummy nature, and the others had been previously mixed with gum, (being cakes of the patent water-colours,) yet, when dried upon paper, they could not be removed with a moistened sponge, until the surface of the paper itself was rubbed off.

In many arts and manufactures, therefore, the solutions of

* Phil. Trans. 1781, p. 380.

lac may be found of much utility; for, like mucilage, they may be diluted with water, and yet, when dry, are little if at all affected by it.*

We find, from the experiments on lac, that this substance is soluble in the alkalis, and in some of the acids. But this fact (considering that resin is the principal ingredient of lac) is in opposition to the generally received opinion of chemists, namely, that acids and alkalis do not act upon resinous bodies. Some experiments, however, which I have made on various resins, gum-resins, and balsams, fully establish, that these substances are powerfully acted upon by the alkalis, and by some of the acids, so as to be completely dissolved, and rendered soluble in water.

It will be a very wide and curious field of inquiry, to discover what changes are thus produced in these bodies, especially by nitric acid. Each substance must form the subject of a separate investigation; and there cannot be a doubt but that much will be learned respecting their nature and properties, which hitherto have been so little examined by chemists.

The alkaline solutions of resin may be found useful in some of the arts; for many colours, especially those which are metallic,

* The alkaline solutions of lac are evidently of a saponaceous nature, and, like other soaps, may be decomposed by acids. The entire substance of lac is not however completely dissolved, as appears from the turbidness of the liquors. Three of the four ingredients, namely, the resin, the gluten, and the colouring extract, appear to be in perfect solution; whilst the wax is only partially combined with the alkali, and forms that imperfectly soluble saponaceous compound which has been formerly mentioned, and which remains suspended, and disturbs the transparency of the solution.

From various circumstances, it does not seem improbable, that the long sought-for, but hitherto undiscovered vehicle employed by the celebrated painters of the Venetian School, may have been some kind of resinous solution, prepared by means of borax, or by the alkalis.

when dissolved in acids, may be precipitated, combined with resin, by adding the former to the alkaline solutions of the latter. I have made some experiments of this kind with success; and perhaps these processes might prove useful to dyers and manufacturers of colours. It is probable also, that medicine may derive advantages from some of this extensive series of alkaline and acid solutions of the resinous substances.

X. *On the Integration of certain differential Expressions, with which Problems in physical Astronomy are connected, &c.* By Robert Woodhouse, *A. M. F. R. S. Fellow of Caius College.*

Read April 12, 1804.

In analytical investigation, two important objects present themselves: the concise and unambiguous expression of the conditions of a problem in algebraic language; and the reduction of such expression into forms commodious for arithmetical computation.

If the introduction of the new calculi, as they have been called, has extended the bounds of science, it has enormously increased its difficulties, in their number and magnitude. The differential forms that can be completely integrated, occur in few problems only, and those of small moment. In physical astronomy, the investigations give rise to differential expressions, which call forth all the resources of the analytic art, even for their approximate integration.

For the integration of differential expressions that, by the process of taking the differential, can be derived from no finite algebraic form, recourse is had to infinite series: thus, if the expression be $dx . fx$, and there is no quantity $\mathrm{F}x$, such that $dx . fx = d(\mathrm{F}x)$: fx is put $= f((x-a)+a) = f(x-a) + \mathrm{D}f(x-a) . a + \underset{c}{\mathrm{D}^2} f(x-a)\, a^2 +$ &c. and $\int dx . fx = \int dx . f(x-a) + \int dx . \mathrm{D}f(x-a) + \int dx . \underset{c}{\mathrm{D}^2} f(x-a)$ &c. or,

putting $fx = f(a + x - a)$, the integral of $dx \,.\, fx$ is calculated from the series $\int dx \,.\, fa \,.\, (x - a) + \int dx \,.\, \mathrm{D}fa \,.\, (x - a)^2 + \&c.$

But, although the integrals of many expressions can thus be exhibited, yet such series are useless for the purpose of arithmetical computation, except their terms continually decrease, and except the limits of the ratio of the decrease of the terms can be determined; and the invention of series adapted to arithmetical computation, has not been the least of the difficulties encountered by modern analysts.

Although the differential expressions that admit no finite integration have not been reduced into classes, yet there are some, from their simplicity, and frequent occurrence in analytical investigation, more conspicuously known and attentively considered: such are the expressions $\frac{dx}{1+x}$, $\frac{dx}{\sqrt{(1-x^2)}}$; and the computation of their integrals, in other words, is the determination of the logarithms of numbers, and the lengths of circular arcs.

The necessity of calculating the integrals of expressions such as $\frac{dx}{1+x}$, $\frac{dx}{\sqrt{(1-x^2)}}$, must soon have obtruded itself on the attention of the early analysts: for several expressions, as $\frac{dx}{1-x^2}$, $\frac{dx}{x\sqrt{1-x^2}}$, $\frac{dx}{1+x^2}$, $\frac{dx}{x\sqrt{x^2-1}}$, &c. apparently dissimilar, are easily reduced to the forms $\frac{dx}{1+x}$, $\frac{dx}{\sqrt{1-x^2}}$; and besides, the difficulty of integrating a variety of forms, is soon reduced to that of the integration of $\frac{dx}{1+x}$, $\frac{dx}{\sqrt{1-x^2}}$: such, for instance, are the forms $\frac{dx}{x\sqrt{(1-x^2)}}$, $\frac{dx}{x^3\sqrt{(1-x^2)}}$, and all that are comprehended under $\frac{dx}{x^{2m+1}\sqrt{(1-x^2)}}$; the forms $\frac{x^2\,dx}{\sqrt{(1-x^2)}}$, $\frac{x^4\,dx}{\sqrt{(1-x^2)}}$, &c. and all that are comprehended under $\frac{x^{2m}\,.\,dx}{\sqrt{(1-x^2)}}$.

It is on the grounds of convenience of calculation, and of systematic arrangement, that differential expressions, such as have been just exhibited, are resolved into a series of terms $Pdx + P'dx + P''dx + \&c. + Q\frac{dx}{\sqrt{1 \pm x^2}}$, where $Pdx + P'dx$, $P''dx$ are integrable; for, remove those grounds, and it will be difficult to assign a reason why * $\frac{x^{2m+1}}{2m+1} + \frac{x^{2m+3}}{2m+3} \cdot \frac{1}{2} + \frac{x^{2m+5}}{2m+5} \cdot \frac{1 \cdot 3}{2 \cdot 4} + \&c.$ is not an integral of $\frac{x^{2m} \cdot dx}{\sqrt{(1-x^2)}}$ equally exact as $-\sqrt{(1-x^2)}\left\{\frac{x^{2m-1}}{2m} + \frac{1 \cdot (2m-1)}{(2m-2) \cdot 2m} \cdot x^{2m-3} + \frac{1 \cdot (2m-1)(2m-3)}{(2m-4)(2m-2) \cdot 2m} \cdot x^{2m-5} + \&c.\right\} + \frac{1 \cdot 3 \cdot 5 \ldots 2m-1}{2 \cdot 4 \cdot 6 \ldots 2m}\int\frac{dx}{\sqrt{(1-x^2)}}$.

In the application of the differential calculus to curve lines, after making certain arbitrary assumptions, it appears that hyperbolic areas, and arcs of circles, may be computed from the integrals of the expressions $\frac{dx}{1+x}$, $\frac{dx}{\sqrt{(1-x^2)}}$; the integrals of which are in fact afforded by the several methods that relate to the quadratures of the circle and hyperbola; and mathematicians, either for the sake of embodying in some degree their speculations, or from a notion of a necessary connexion subsisting between circles, hyperbolas, and the integrals of $\frac{dx}{1+x}$, $\frac{dx}{\sqrt{(1-x^2)}}$, have expressed the integrals by the arcs and areas of those figures. Although the computation of the integrals, is totally independent of the existence of the figures, and of their properties, yet it is curious, that the simplest transcendental expressions of analysis, should express parts of the simplest figures in geometry.

* This series arises from expanding $\frac{1}{\sqrt{1-x^2}}$, and from integrating each term multiplied into x^{2m}. dx.

In analytical investigation, after $\frac{dx}{1+x}$, $\frac{dx}{\sqrt{(1-x^2)}}$, the transcendental expression, next, in point of simplicity, is $dx\sqrt{\left(\frac{1-e^2x^2}{1-x^2}\right)}$;* in a particular application, this differential represents the arc of an ellipse,† a figure, next, in point of simplicity, to the circle.

Many differential expressions depending, for their integration, on the integral of $dx\sqrt{\left(\frac{1-e^2x^2}{1-x^2}\right)}$, it became necessary to exhibit it, for all values of x and e. A problem in consequence arose, of no small difficulty, named, analogously to the naming of $\int\frac{dx}{\sqrt{1-x^2}}$, the rectification of the ellipse. In the prosecution of the researches to which this problem led, it was discovered that the hyperbola might be rectified by means of the ellipse, or, to speak correctly, and without the employment of figurative language, it was discovered that the transcendental expression $dx\sqrt{\left(\frac{e^2x^2-1}{x^2-1}\right)}$ $(e > 1)$ might be made to depend, for its integration, on that of $dx\sqrt{\left(\frac{1-e^2x^2}{1-x^2}\right)}$ $e < 1$.

The integration of $dx\sqrt{\left(\frac{1-e^2x^2}{1-x^2}\right)}$ does not depend more on the length of an ellipse, than it does, on the time of the vibration of a pendulum in a circular arc, or on the attraction of a spheroid; but, in each of these problems, it occurs as an analytical phrase, an expression in symbolical language, the exact meaning of which it is necessary to know. If the meaning be determined for one case, it is for all three; and hence, with the rectification of

* $\frac{dx}{\sqrt{(1-x^2)(1-e^2x^2)}}$ is as simple an expression: they are considered together in the following pages.

† The ellipse admits of an easy mechanical description; and, considered as a section of the cone, was admitted by the ancient geometricians into plane geometry.

the ellipse, a problem by itself unimportant, the solutions of other problems, are intimately connected; and, with this object in view, the determination of the length of a curve line, mathematicians have enriched analysis with several curious artifices, and valuable methods.

To determine the integrals of $\frac{dx}{\sqrt{1-x^2}}$, $dx\sqrt{\left(\frac{1-e^2x^2}{1-x^2}\right)}$, it is necessary to expand them into series. The difficulty is, to expand them into series that converge: the determination of the integral of $\frac{dx}{\sqrt{(1-x^2)}}$ ought to precede that of $dx\sqrt{\left(\frac{1-e^2x^2}{1-x^2}\right)}$; indeed, in most of the series that represent the latter, $\int\frac{dx}{\sqrt{(1-x^2)}}$ is involved as a term, and is supposed to be known. The determination of each integral presents a curious circumstance, in the correspondence of certain geometrical properties and analytical artifices; for instance, the theorem for the tangent of the sum of two circular arcs, affords, analytically, a means of computing the length of the arc; and, conversely, the analytical artifice* by which the integral of $\frac{dx}{\sqrt{(1-x^2)}}$ is computed, translated, leads to the

* The method of deducing the value of $\int\frac{dx}{\sqrt{1-x^2}}$ between the values of x, 0 and 1, independently of any reference to a circle, is as follows.

Let $\frac{dx}{\sqrt{1-x^2}}=\frac{du'}{\sqrt{1-u'^2}}+\frac{du''}{\sqrt{1-u''^2}}$ then $\int\frac{dx}{\sqrt{(1-x^2)}}=\int\frac{du'}{\sqrt{(1-u'^2)}}+\int\frac{du''}{\sqrt{(1-u''^2)}}+C$, and, expressing the integrals by their exponential expressions, we may deduce (see Phil. Trans. 1802) $u'\sqrt{(1-u''^2)}+u''\sqrt{(1-u'^2)}=x$. Let $x=1$ and $u'=u''=\therefore\frac{1}{\sqrt{2}}$, consequently $\frac{2du'}{\sqrt{(1-u'^2)}}=\frac{dx}{\sqrt{(1-x^2)}}$, or twice the integral of $\frac{du'}{\sqrt{(1-u'^2)}}$ between the values of u', 0 and $\frac{1}{\sqrt{2}}$, equals the integral of $\frac{dx}{\sqrt{(1-x^2)}}$ between the values of x, 0 and 1. Again, put $\frac{du'}{\sqrt{(1-u'^2)}}=\frac{dv}{\sqrt{(1-v^2)}}+\frac{dv'}{\sqrt{(1-v'^2)}}$ $\therefore$ as before, $v\sqrt{(1-v'^2)}+v'\sqrt{(1-v^2)}=u'$; put $u'=\frac{1}{\sqrt{2}}$, $v=\frac{1}{\sqrt{5}}$ and $v'=\frac{1}{\sqrt{10}}$, consequently $\int\frac{dx}{\sqrt{(1-x^2)}}=2\int\frac{dv'}{\sqrt{(1-v'^2)}}$ (contained between the values of v', 0 and $\frac{1}{\sqrt{10}}$) +

properties of the sines, and tangents, of circular arcs. Again, Fagnani's theorem, by which a right line is assigned equal to the difference of two elliptic arcs, affords a method of arithmetically computing the length of the ellipse; and, conversely, the analytical artifice by which the integral of $dx\sqrt{\left(\frac{1-e^2x^2}{1-x^2}\right)}$ is computed; translated into geometrical language, becomes Fagnani's theorem. And again, the analytical resolution of $\int dx\sqrt{\left(\frac{1-e^2x^2}{1-x^2}\right)}$ into $Au' + Bu'' + P\int du'\sqrt{\left(\frac{1-e'^2u'^2}{1-u'^2}\right)} +$ $Q\int du''\sqrt{\left(\frac{1-e''^2u''^2}{1-u''^2}\right)}$, (where the integrals, on account of the

$2\int\frac{dv}{\sqrt{(1-v^2)}}$ (contained between the values of v, 0 and $\frac{1}{\sqrt{5}}$), which latter series, from the smallness of v, v', converge with considerable rapidity; or the latter part thus, put $v = \frac{z}{\sqrt{(1+z^2)}}$, $v' = \frac{z'}{\sqrt{(1+z'^2)}}$, $u' = \frac{y}{\sqrt{(1+y^2)}}$, then $\frac{z+z'}{1-zz'} = y$, and $\int\frac{dy}{1+y^2}$ $= \int\frac{dz}{1+z^2} + \int\frac{dz'}{1+z'^2}$.

Now, if $u' = \frac{1}{\sqrt{2}}$, $y = 1$,

if $v = \frac{1}{\sqrt{5}}$, $z = \frac{1}{2}$,

if $v' = \frac{1}{\sqrt{10}}$, $z' = \frac{1}{3}$.

Consequently, the integral of $\frac{dy}{1+y^2}$ (between the values of y, 0 and 1) $= \int\frac{dz}{1+z^2}$ (between the values of z, 0 and $\frac{1}{2}$) $+ \int\frac{dz'}{1+z'^2}$ (between the values of z', 0 and $\frac{1}{3}$), and consequently, $\int\frac{dx}{\sqrt{(1-x^2)}}$ (between 0 and 1) $= 2\left\{\frac{1}{2} - \frac{1}{3.2^3} + \frac{1}{5.2^5} - \&c.\right\}$ $+ 2\left\{\frac{1}{3} - \frac{2}{3.3^3} + \frac{1}{5\,3^5} - \&c.\right\}$ which is, in fact, Euler's method of determining the periphery of a circle. Now, from this analytical artifice of putting the integral of $\frac{dx}{\sqrt{(1-x^2)}} = \int\frac{du'}{\sqrt{(1-u'^2)}} + \int\frac{du''}{\sqrt{(1-u''^2)}}$, by which means its arithmetical value is computed, may be deduced those theorems which relate to the sines, and tangents, of the sum and difference of arcs, &c. by translating the formula $u'\sqrt{(1-u''^2)} + u''\sqrt{(1-u'^2)} = x$ into geometrical language.

smallness of e', e'', are readily computed,) translated into the language of geometry, expresses a curious relation between the arcs of three ellipses, the excentricities of which vary according to a certain law.

Hence it appears, that there are two different methods by which the analytic art may be advanced; either by artifices peculiarly its own, or by aid drawn from the properties of figures and curve lines; if, for instance, FAGNANI's theorem be proved for an ellipse, by processes purely geometrical, then, such a theorem, expressed in analytical language, becomes immediately a means of computing the integral of $dx\sqrt{\frac{1-e^2x^2}{1-x^2}}$; or if, by reasonings strictly geometrical, a relation can be established between the arcs of three ellipses, whose excentricities vary according to a certain law, then, by expressing such a relation in the signs of algebra, the integral of $dx\sqrt{\left(\frac{1-e^2x^2}{1-x^2}\right)}$ may be computed by means of the integrals of $du'\sqrt{\left(\frac{1-e'^2u'^2}{1-u'^2}\right)}$, and of $du''\sqrt{\left(\frac{1-e''^2u''^2}{1-u''^2}\right)}$; which integrals can be found more readily than the original integral, by reason of the quicker convergency of the series into which the differential expressions may be expanded, e' and e'' being less than e.

One main object of the present paper is, to exhibit the integral of $dx\sqrt{\left(\frac{1-e^2x^2}{1-x^2}\right)}$ for all values of e, and to reduce other integrals to it. Much has been already done on this subject. The researches of mathematicians on the length and comparison of elliptic arcs, are extended over the surface of many memoirs; yet I hope to have something to add in point of invention, and more in point of arrangement and simplicity of expression. The labours of future students will surely be lessened, if it be

shown, that several methods, apparently distinct and dissimilar, because expressed in different language, are fundamentally, and in principle, the same.

The simplest mode, and the first that occurred to mathematicians, of finding the value of $\int dx \sqrt{\left(\frac{1-e^2x^2}{1-x^2}\right)}$ was, to expand the differential expression into a series of terms ascending by the powers of e, and to take the integral of each term. This method, however, is very imperfect; for, if e be nearly $= 1$, the series converges so slowly as to be unfit, or at least very incommodious, for arithmetical computation. It became necessary then to possess a series ascending by the powers of $1 - e^2$; and such a series was first given by EULER, in his Opuscula, published at Berlin in 1750; and it must be manifest, that there can be no one single series, ascending by the powers of e, or by powers of the same function e, that can in all cases represent its value. I purpose to consider the several series that represent the value of $\int dx \sqrt{\left(\frac{1-e^2x^2}{1-x^2}\right)}$,

when e is small,

when e is nearly $= 1$, or, when $\sqrt{(1-e^2)}$ is small,

when e is $\angle \sqrt{(1-e^2)}$ and $\angle \frac{1}{\sqrt{2}}$,

when e is $> \sqrt{(1-e^2)}$ and $> \frac{1}{\sqrt{2}}$,

when e and $\sqrt{(1-e^2)}$ are equal, or when each equals $\frac{1}{\sqrt{2}}$.

The series for the first and second cases, I shall deduce, because I wish to consider the subject in its fullest extent; but those series, when we regard practical commodiousness, are superseded by the methods by which the $\int dx \sqrt{\left(\frac{1-e^2x^2}{1-x^2}\right)}$ is to be found, in the third and fourth cases. Two methods then, are only requisite for finding the integral in all the values of e; for the integral

in the last case may be found, with nearly equal convenience, by either of the methods in the two preceding cases.

For the sake of conciseness, I employ the symbol D to denote the numeral coefficients of the terms arising from the expansion of $(1-x)^m$; thus, $D1^m$ signifies m; $D^2 1^m, m.m-1$; $\underset{c}{D}^2 1^m$, $\frac{m.m-1}{1.2}$; $\underset{c}{D}^3 1^m, \frac{m.(m-1)(m-2)}{1.2.3}$; $\underset{c}{D}^n 1^m$ signifies, $\frac{m.(m-1)(m-2)..m-n+1)}{1.2.3...n}$, and consequently, in particular values of m and n, $\underset{c}{D}^3 1\frac{1}{2}$ signifies $\frac{1.1.3}{2.4.6}$; $\underset{c}{D}^4 1\frac{1}{2}$ signifies $-\frac{1.1.3.5}{2.4.6.8}$; $\underset{c}{D}^3 1^{-\frac{1}{2}}$ signifies $-\frac{1.3.5}{2.4.6}$; $\underset{c}{D}^4 1^{-\frac{1}{2}}$, signifies $\frac{1.3.5.7}{2.4.6.8}$; &c.

Employing, therefore, this notation in the expansion of $\sqrt{(1-e^2x^2)}$, we have $dx\sqrt{\left(\frac{1-e^2x^2}{1-x^2}\right)} = \frac{dx}{\sqrt{1-x^2}}\Big\{1\frac{1}{2} - D1\frac{1}{2}e^2x^2 + \underset{c}{D}^2 1\frac{1}{2}e^4x^4 - \underset{c}{D}^3 1\frac{1}{2}e^6x^6 + \&c.\Big\}$, and the $(n+1)$th term is

$$\underset{c}{D}^n 1\tfrac{1}{2}e^{2n}.\frac{x^{2n}.dx}{\sqrt{(1-x^2)}}.$$

Now, $d(x^{2n-1}\sqrt{(1-x^2)}) = (2n-1)\frac{x^{2n-2}dx}{\sqrt{(1-x^2)}} - \frac{2nx^{2n}.dx}{\sqrt{(1-x^2)}}$; hence,

$$\int\frac{x^{2n}dx}{\sqrt{(1-x^2)}} = -\frac{1}{2n}x^{2n-1}\sqrt{(1-x^2)} + \frac{2n-1}{2n}\int\frac{x^{2n-2}dx}{\sqrt{(1-x^2)}}$$
$$= -\frac{1}{2n}x^{2n-1}\sqrt{(1-x^2)} - \frac{2n-1}{2n.2n-2}x^{2n-3}\sqrt{(1-x^2)}$$
$$+\frac{(2n-1).(2n-3)}{2n.(2n-2)}.\int\frac{x^{2n-4}dx}{\sqrt{(1-x^2)}};$$

consequently, continuing the reduction,

$$\int\frac{x^{2n}dx}{\sqrt{(1-x^2)}} = -\sqrt{(1-x^2)}\Big\{\frac{x^{2n-1}}{2n} + \frac{(2n-1)}{2n.(2n-2)}.x^{2n-3} + \&c.\Big\}$$
$$+\frac{(2n-1).(2n-3)\ \&c. \ldots\ldots 5.3.1}{2n.2n-2\ldots\ldots\ldots 6.4.2}\int\frac{dx}{\sqrt{(1-x^2)}}\ (\varphi).$$

Hence, putting for n the several values 0, 1, 2, 3, &c. we have

$$\int dx\sqrt{\left(\frac{1-e^2x^2}{1-x^2}\right)} = \varphi$$
$$-D1\tfrac{1}{2}e^2\Big\{\frac{-x\sqrt{(1-x^2)}}{2} + \frac{\varphi}{2}\Big\}$$
$$+\underset{c}{D}^2 1\tfrac{1}{2}e^4\Big\{\frac{-x^3\sqrt{(1-x^2)}}{4} - \frac{3x\sqrt{(1-x^2)}}{4.2} + \frac{1.3}{2.4}\varphi\Big\}$$

$$-\underset{c}{\mathrm{D}}^3\, 1^{\frac{1}{2}} e^6 \left\{ \frac{-x^5\sqrt{(1-x^2)}}{6} - \frac{5x^3\sqrt{(1-x^2)}}{6\,.\,4} - \frac{5\,.\,3\,.\,x\sqrt{(1-x^2)}}{6\,.\,4\,.\,2} + \frac{1\,.\,3\,.\,5\,.\,\varphi}{2\,.\,4\,.\,6} \right\}$$

+ &c.

Hence, if the integral of $dx\sqrt{\left(\frac{1-e^2x^2}{1-x^2}\right)}$, between the values of x, o and 1, be required, putting $\frac{\pi}{2} = \frac{3\,.\,14159}{2}$ = value of φ, or of $\int\frac{dx}{\sqrt{(1-x^2)}}$, when $x = 1$, we have

$$\int dx\sqrt{\left(\frac{1-e^2x^2}{1-x^2}\right)} \text{ (from } x = 0 \text{ to } x = 1)$$

$$= \frac{\pi}{2}\left\{1 - \mathrm{D}1^{\frac{1}{2}}.e^2.\tfrac{1}{2} + \underset{c}{\mathrm{D}}^2\, 1^{\frac{1}{2}}.\,e^4.\,\frac{1\,.\,3}{2\,.\,4} - \underset{c}{\mathrm{D}}^3\, 1^{\frac{1}{2}} e^6.\,\frac{1\,.\,3\,.\,5}{2\,.\,4\,.\,6} + \text{\&c.}\right\}(1)$$

or, developing the symbolical coefficients $\mathrm{D}1^{\frac{1}{2}}$, $\mathrm{D}^2\, 1^{\frac{1}{2}}$, &c.

$$= \frac{\pi}{2}\left\{1 - \frac{1\,.\,1}{2\,.\,2}e^2 - \frac{1\,.\,1}{2\,.\,4}.\,\frac{1\,.\,3}{2\,.\,4}e^4 - \frac{1\,.\,1\,.\,3}{2\,.\,4\,.\,6}.\,\frac{1\,.\,3\,.\,5}{2\,.\,4\,.\,6}e^6 - \text{\&c.}\right\}$$

which series has been given by several authors, SIMPSON, EULER, *Animadversiones in Rect. Ellips.* p. 129, &c.

If, instead of the coefficients $\frac{1}{2}.\,\frac{1\,.\,3}{2\,.\,4}$, we use $-\mathrm{D}1^{-\frac{1}{2}}, \underset{c}{\mathrm{D}}^2\, 1^{-\frac{1}{2}}$, &c. the integral

$$= \frac{\pi}{2}\left\{1 + \mathrm{D}1^{\frac{1}{2}}.\,\mathrm{D}1^{-\frac{1}{2}}.\,e^2 + \underset{c}{\mathrm{D}}^2\, 1^{\frac{1}{2}}.\,\underset{c}{\mathrm{D}}^2\, 1^{-\frac{1}{2}}.\,e^4 + \underset{c}{\mathrm{D}}^3\, 1^{\frac{1}{2}}.\,\underset{c}{\mathrm{D}}^3\, 1^{-\frac{1}{2}}.\,e^6 + \text{\&c.}\right\}$$

where the $(n+1)$th term is $\underset{c}{\mathrm{D}}^n\, 1^{\frac{1}{2}}.\,\underset{c}{\mathrm{D}}^n\, 1^{-\frac{1}{2}}$, which, (since $\underset{c}{\mathrm{D}}^n\, 1^{-\frac{1}{2}} = -\underset{c}{\mathrm{D}}^n\, 1^{\frac{1}{2}}.\,(2n-1)$), equals $-\left(\underset{c}{\mathrm{D}}^n\, 1^{\frac{1}{2}}\right)^2.\,(2n-1)$; consequently, the integral may be put

$$\frac{\pi}{2}\left\{1 - \left(\mathrm{D}1^{\frac{1}{2}}\right)^2.\,e^2 - \left(\underset{c}{\mathrm{D}}^2\, 1^{\frac{1}{2}}\right)^2.\,3e^4 - \left(\underset{c}{\mathrm{D}}^3\, 1^{\frac{1}{2}}\right)^3.\,5e^6 - \text{\&c.}\right\}$$

From this series, $\int dx\sqrt{\left(\frac{1-e^2x^2}{1-x^2}\right)}$ may be computed when e is small; but it is evidently of very little use when e is either nearly = 1, or is of mean value. To speak in geometrical language, the length of an ellipse of small excentricity may be computed by the above series.

If v be put $= 1 - 2x^2$, $\frac{dx}{\sqrt{(1-x^2)}} = \frac{-dv}{2\sqrt{(1-v^2)}}$, and $dx\sqrt{\left(\frac{1-e^2x^2}{1-x^2}\right)} = \frac{-dv}{2\sqrt{(1-v^2)}}.\sqrt{\frac{(2-e^2)}{2}}\sqrt{\left(1+\frac{e^2v}{2-e^2}\right)}$,

put $\frac{e^2}{2-e^2}=c$, and $dx\sqrt{\left(\frac{1-e^2x^2}{1-x^2}\right)}=df$,

then $df=\frac{1}{2}\sqrt{\frac{e^2}{2c}}\cdot\frac{dv}{\sqrt{(1-v^2)}}\left\{1^{\frac{1}{2}}+\text{D}\,1^{\frac{1}{2}}.cv+\underset{c}{\text{D}^2}\,1^{\frac{1}{2}}.c^2v^2+\&c.\right\}$

Now (by methods similar to those that have been given)

$$\int\frac{v^{2n}\,dv}{\sqrt{(1-v^2)}}=-\sqrt{(1-v^2)}\left\{\frac{v^{2n-1}}{2n}+\frac{2n-1v^{2n-3}}{2n\,.\,(2n-2)}+\&c.\right\}+\frac{1\,.\,3\ldots.2n-1}{2\,.\,4\ldots.2n}$$

$\int\frac{dv}{\sqrt{1-v^2}}$, and $\int\frac{v^{2n+1}\,dv}{\sqrt{(1-v^2)}}=-\sqrt{1-v^2)}\left\{\frac{v^{2n}}{2n+1}+\frac{1\,.\,2n\,v^{2n-2}}{(2n+1)\,(2n-1)}\right.$.

$\left.+\&c.+\frac{1\,.\,2\,.\,4\ldots.\,2n}{1\,.\,3\,.\,5\ldots.\,2n+1}\right\}$.

Hence, putting $\phi'=\int\frac{-dv}{\sqrt{(1-v^2)}}$

$$f=\frac{1}{2}\sqrt{\frac{e^2}{2c}}\left\{\begin{array}{l}\phi'+\text{D}\,1^{\frac{1}{2}}c\sqrt{(1-v^2)}+\underset{c}{\text{D}^2}\,1^{\frac{1}{2}}c^2\left\{\frac{v\sqrt{(1-v^2)}}{2}+\frac{\phi'}{2}\right\}\\+\underset{c}{\text{D}^3}\,1^{\frac{1}{2}}.c^3\left\{.\frac{v^2}{3}\sqrt{(1-v^2)}+\frac{1\,.\,2}{2\,.\,4}\right\}+\&c.\end{array}\right\}$$

which series agrees exactly with LEGENDRE's, given in *Mem. de l'Acad.* p. 620, when the quantities v, $\sqrt{1-v^2}$, &c. are expressed in geometrical language.

In order to find the integral from $x=0$ to $x=1$, put $x=0$, then $v=1$, put $x=\frac{1}{\sqrt{2}}$, and then $v=0$; but it has appeared that the $\int\frac{dx}{\sqrt{(1-x^2)}}$ (between the values of x, 0 and 1) $=2\int\frac{dx}{\sqrt{(1-x^2)}}$ (between the values of x, 0 and $\frac{1}{\sqrt{2}}$) $=$ consequently, $2\int\frac{dv}{\sqrt{(1-v^2)}}$ (between the values of v, 1 and 0).

$$\text{Hence, }f=\sqrt{\left(\frac{2-e^2}{2}\right)}\frac{\pi}{2}\left\{1+\underset{c}{\text{D}^2}\,1^{\frac{1}{2}}.\frac{c^2}{2}+\underset{c}{\text{D}^4}\,1^{\frac{1}{2}}.\frac{1\,.\,3}{2\,.\,4}.c^4+\&c.\right\}\quad(2)$$

$$\text{or}=\sqrt{\left(1-\frac{e^2}{2}\right)}\frac{\pi}{2}\left\{1-\frac{1\,.\,1}{2\,.\,4}.\frac{1}{2}.c^2-\frac{1\,.\,1\,.\,3\,.\,5}{2\,.\,4\,.\,6\,.\,8}.\frac{1\,.\,3}{2\,.\,4}c^4-\&c.\right\}$$

which is the series given by LEGENDRE, and by EULER, *Novi Comm. Petrop.* Tom. XVIII. p. 71, and called by that author *Series maxume convergens;* yet the series is by no means practically commodious when e is nearly 1.

A very useful series, when e is small, was given by Mr. IVORY,

in the Edinburgh Transactions, Vol. IV. which I shall notice in the sequel; not now, because I consider it as a particular case of the general method by which, in all cases, the integral of $dx\sqrt{\left(\frac{1-e^2x^2}{1-x^2}\right)}$ may be computed.

In order to deduce the series by which, when e is nearly $=1$, f may be computed, put $1-e^2=b^2$,

then $df=dx\sqrt{\left(1+\frac{(1-e^2)x^2}{1-x^2}\right)}=dx\sqrt{\left(1+\frac{b^2x^2}{1-x^2}\right)}=\left(\text{if } x=\frac{z}{\sqrt{(1+z^2)}}\right)$

$$\frac{dz}{(1+z^2)^{\frac{3}{2}}}\sqrt{(1+b^2z^2)}=d\left\{\frac{z}{\sqrt{1+z^2}}\sqrt{(1+b^2z^2)}\right\}-\frac{b^2z^2dz}{\sqrt{1+z^2}\sqrt{(1+b^2z^2)}}.$$

Now, $\frac{b^2z^2dz}{\sqrt{((1+z^2)(1+b^2z^2))}}=\frac{b^2z^2dz}{\sqrt{(1+z^2)}}\left\{1^{-\frac{1}{2}}+\mathrm{D}1^{-\frac{1}{2}}b^2z^2+\underset{c}{\mathrm{D}^2}1^{-\frac{1}{2}}b^4z^4+\&c.\right\}$

and the $(n+1)$th term $=\underset{c}{\mathrm{D}^n}1^{-\frac{1}{2}}b^{2n+2}.\frac{z^{2n+2}.dz}{\sqrt{(1+z^2)}}$.

Now, $\int\frac{z^{2n+2}.dz}{\sqrt{(1+z^2)}}=\frac{z^{2n+1}\sqrt{(1+z^2)}}{2n+2}-\frac{2n+1}{2n+2}\int\frac{z^{2n}dz}{\sqrt{(1+z^2)}}$; and, consequently,

$$=\frac{z^{2n+1}\sqrt{(1+z^2)}}{2n+2}-\frac{2n+1}{2n+2\,.\,2n}.z^{2n-1}\sqrt{(1+z^2)}+\frac{(2n+1)(2n-1)}{(2n+2)\,2n}\int\frac{z^{2n-2}dz}{\sqrt{1+z^2}}$$

$$=\sqrt{(1+z^2)}\left\{\frac{z^{2n+1}}{2n+2}-\frac{2n+1\,.\,z^{2n-1}}{2n+2\,.\,2n}+\frac{2n+1)(2n-1)}{(2n+2)\,2n\,.\,(2n-2)}.z^{2n-3}+\&c.\right\}$$

$$\pm\frac{(2n+1)(2n-1)\ldots\ldots3\,.\,1}{(2n+2)\,2n\,.\ldots\ldots4\,.\,2}.\int\frac{dz}{1+z^2}\left(\underset{c}{\mathrm{D}^{n+1}}1^{-\frac{1}{2}}.\int\frac{dz}{\sqrt{(1+z^2)}}\right).$$

Hence, $\int\frac{dz}{(1+z^2)^{\frac{3}{2}}}\sqrt{(1+b^2z^2)}$

$=\frac{z}{\sqrt{(1+z^2)}}\sqrt{(1+b^2z^2)}$

$-\left\{\mathrm{D}1^{-\frac{1}{2}}b^2+\mathrm{D}1^{-\frac{1}{2}}\underset{c}{\mathrm{D}^2}1^{-\frac{1}{2}}.b^4+\underset{c}{\mathrm{D}^2}1^{-\frac{1}{2}}\underset{c}{\mathrm{D}^3}1^{-\frac{1}{2}}b^6+\underset{c}{\mathrm{D}^3}1^{-\frac{1}{2}}\underset{c}{\mathrm{D}^4},1^{-\frac{1}{2}}.b^8+\&c.\right\}$

$\times\log.(z+\sqrt{(1+z^2)})$

$-b^2.\frac{z\sqrt{(1+z^2)}}{2}$

$-\mathrm{D}1^{-\frac{1}{2}}b^4\sqrt{(1+z^2)}\left\{\frac{z^3}{4}-\frac{3z}{4\,.\,2}\right\}$

$-\underset{c}{\mathrm{D}^2}1^{-\frac{1}{2}}b^6\sqrt{(1+z^2)}\left\{\frac{z^5}{6}-\frac{5z^3}{6\,.\,4}+\frac{5\,.\,3\,.\,z}{6\,.\,4\,.\,2}-\&c.\right\}$

$-$ &c.

or, since $\sqrt{(1+z^2)}=\frac{1+z^2}{\sqrt{(1+z^2)}}$

$\int \frac{dz}{\sqrt{(1+z^2}} \sqrt{(1+b^2 z^2)}$

$= \frac{z\sqrt{(1+b^2 z^2)}}{\sqrt{(1+z^2)}}$

$-\left\{ D1^{-\frac{1}{2}} b^2 + D1^{-\frac{1}{2}} \underset{c}{D}^2 1^{-\frac{1}{2}} b^4 + \underset{c}{D}^2 1^{-\frac{1}{2}} . \underset{c}{D}^3 1^{-\frac{1}{2}} \right\} \left\{ l.z + \sqrt{(1+z^2)} \right.$

$\left. - \frac{z}{\sqrt{(1+z^2)}} \right\}$

$- \frac{b^2 z}{\sqrt{1+z^2}} \left\{ \frac{z^2}{2} \right\}$

$- D1^{-\frac{1}{2}} \frac{b^4 . z}{\sqrt{(1+z^2)}} \left\{ \frac{z^4}{4} - \frac{z^2}{4.2} \right\}$

$- \underset{c}{D}^2 1^{-\frac{1}{2}} . \frac{b^6 . z}{\sqrt{(1+z^2)}} \left\{ \frac{z^6}{6} - \frac{z^4}{6.4} + \frac{z^2}{6.4.2} \right\}$

— &c.

Now, from this series, as it stands, the whole integral of $dx\sqrt{\left(\frac{1-e^2 x^2}{1-x^2}\right)}$ cannot be computed, because x being $= \frac{z}{\sqrt{(1+z^2)}}$ when $x=1$, z is infinite: therefore, we must use an artifice similar to that by which $\int \frac{dx}{\sqrt{(1-x^2)}}$ has been computed; which artifice consists in finding v a function of x, such that $\int \frac{dx}{\sqrt{1-x^2}}$ (between $x = 0$ and $x = a$, $a \angle 1$) $+ \int \frac{dv}{\sqrt{1-v^2}}$ shall $=$ whole integral of $\frac{dx}{\sqrt{(1-x^2)}} \{$from $x = 0$ to $x = 1.\}$

Let therefore $x = \sqrt{\left(\frac{1-v^2}{1-e^2 v^2}\right)}$, in which case, $\frac{x\sqrt{(1-x^2)}}{\sqrt{(1-e^2 x^2)}} = \frac{v\sqrt{(1-v^2)}}{\sqrt{(1-e^2 v^2)}}$; consequently, $dx\sqrt{\left(\frac{1-e^2 x^2}{1-x^2}\right)} = -\frac{dv\,(1-e^2)}{\sqrt{(1-v^2)}\,(1-e^2 v^2)^{\frac{3}{2}}}$,

and $dx\sqrt{\left(\frac{1-e^2 x^2}{1-x^2}\right)} + dv\sqrt{\left(\frac{1-e^2 v^2}{1-v^2}\right)} = e^2\,dv \frac{(1-2v^2+e^2 v^4)}{\sqrt{(1-v^2)}\,(1-e^2 v^2)^{\frac{3}{2}}}$

$= e^2 \times d\left\{ \frac{v\sqrt{(1-v^2)}}{\sqrt{(1-e^2 v^2)}} \right\}.$

Hence,

$\int dx\sqrt{\left(\frac{(1-e^2 x^2)}{(1-x^2)}\right)}\,(f) + \int dv\sqrt{\left(\frac{1-e^2 v^2}{1-v^2}\right)} = \frac{e^2 . v\sqrt{(1-v^2)}}{\sqrt{(1-e^2 v^2)}} + \text{Corr (C)}$ when $x = 1$, $v = 0$. Let the whole integral of $dx\sqrt{\left(\frac{1-e^2 x^2}{1-x^2}\right)}$, from $x = 0$ to $x = 1$, be denoted by $f(1)$ ∴ $C = f(1)$,

and $f + \int dv \surd \left(\frac{1-e^2 v^2}{1-v^2}\right) = f(1) + \frac{e^2 \cdot v\surd(1-v^2)}{\surd(1-e^2 v^2)}$.

Now, $\frac{e^2 v\surd(1-v^2)}{\surd(1-e^2 v^2)} = 0$, both when v is 0 and when v is 1; consequently, there is an intermediate value of v, with which $\frac{e^2 v\surd(1-v^2)}{\surd(1-e^2 v^2)}$ is a maximum. Such value of v, investigated, appears to be $\frac{1}{\surd(1+\surd(1-e^2))}$ = also x; consequently, $2f = f(1) + 1 - \surd(1-e^2)$ $= f(1) + 1 - b$.*

Now, from this property of the integral of $dx \surd \left(\frac{1-e^2 x^2}{1-x^2}\right)$, may the whole integral be computed; for, since $x = \frac{1}{\surd(1+b)}$, $z = \frac{1}{\surd b}$; consequently, $f(1) = 2f - 1 + b$

$$= 1 + b \quad \left(\text{for, putting } z = \frac{1}{\surd b},\ z\surd\left(\frac{1+b^2 z^2}{1+z^2}\right) = 1\right)$$

$$- 2\left\{ D1^{-\frac{1}{2}} b^2 + D1^{-\frac{1}{2}} . \underset{c}{D}^2 1^{-\frac{1}{2}} b^4 + \underset{c}{D}^2 1^{-\frac{1}{2}} . \underset{c}{D}^3 1^{-\frac{1}{2}} b^6 + \&c. \right\}$$

$$\left\{ l. \frac{1+\surd(1+b)}{\surd b} - \frac{1}{\surd(1+b)} \right\}$$

$$- \frac{2}{\surd(1+b)} \cdot \frac{b}{2}$$

$$- \frac{2D1^{-\frac{1}{2}}}{\surd(1+b)} \left\{ \frac{b^2}{4} - \frac{b^3}{4 \cdot 2} \right\} \qquad (3)$$

$$- \frac{2\underset{c}{D}^2 1^{-\frac{1}{2}}}{\surd(1+b)} \left\{ \frac{b^3}{6} - \frac{b^4}{6 \cdot 4} + \frac{b^5}{6 \cdot 4 \cdot 2} \right\}$$

$$- \&c.$$

This form is, in fact, the same as what is given by LEGENDRE, *Mem. de l'Acad.* 1786; and, if the integral had been taken by a method a little different from the above, a series exactly coinciding with LEGENDRE's would have resulted. Thus,

$$\text{since } df = dz \frac{\surd(1+b^2 z^2)}{(1+z^2)^{\frac{3}{2}}} = \frac{dz}{(1+z^2)^{\frac{3}{2}}} \left\{ 1^{\frac{1}{2}} + D1^{\frac{1}{2}} . b^2 z^2 + \underset{c}{D}^2 1^{\frac{1}{2}} b^4 z^4 + \&c. \right\}$$

* I have, in a succeeding page, deduced this theorem of FAGNANI from the general method, contained in the following pages, for computing $\int dx \sqrt{\frac{1-e^2 x^2}{1-x^2}}$.

the $(n+1)$th term $= \underset{c}{\mathrm{D}}^n 1\tfrac{1}{2} \cdot \frac{b^{2n} z^{2n} \cdot dz}{(1+z^2)^{\frac{3}{2}}} = \underset{c}{\mathrm{D}}^n 1\tfrac{1}{2} \cdot b^{2n} \cdot (2n-1) \frac{z^{2n-2}\, dz}{\sqrt{(1+z^2)}}$
$- \underset{c}{\mathrm{D}}^n 1\tfrac{1}{2}\, b^{2n} \times d\left(\frac{z^{2n-1}}{\sqrt{(1+z^2)}}\right)$. Now, if the integrals of
$\underset{c}{\mathrm{D}}^n 1\tfrac{1}{2}\, b^{2n} (2n-1) \frac{z^{2n-2}\, dz}{\sqrt{(1+z^2)}}$ be taken and added together, for the several values of n, (similarly to what has been already done,) there results,

$$f = {}^{*}\left\{\mathrm{D}1\tfrac{1}{2} \cdot b^2 + \underset{c}{\mathrm{D}}^2\, 1\tfrac{1}{2} \cdot \mathrm{D}1^{-\frac{3}{2}} \cdot b^4 + \underset{c}{\mathrm{D}}^3\, 1\tfrac{1}{2} \cdot \underset{c}{\mathrm{D}}^2\, 1^{-\frac{3}{2}} \cdot b^6 + \&c.\right\}$$
$$\left\{ l.\, z + \sqrt{(1+z^2)} - \frac{z}{\sqrt{(1+z^2)}} \right\}$$
$$+ \underset{c}{\mathrm{D}}^2\, 1\tfrac{1}{2}\, b^4 \cdot \frac{z}{\sqrt{(1+z^2)}} \left(\frac{z^2}{2}\right)$$
$$+ \underset{c}{\mathrm{D}}^3\, 1\tfrac{1}{2}\, b^6 \cdot \frac{z}{\sqrt{(1+z^2)}} \left\{\frac{z^4}{4} - \frac{5 \cdot z^2}{4 \cdot 2}\right\}$$
$$+ \underset{c}{\mathrm{D}}^4\, 1\tfrac{1}{2}\, b^8 \cdot \frac{z}{\sqrt{(1+z^2)}} \left\{\frac{z^6}{6} - \frac{5z^4}{6 \cdot 4} + \frac{5 \cdot 3 \cdot z^2}{6 \cdot 4 \cdot 2}\right\}$$
$$+ \&c.$$

consequently, putting $z = \frac{1}{\sqrt{b}}$, we have $f(1) = 2f - 1 + b$

$$= 2\left(\mathrm{D}1\tfrac{1}{2}\, b^2 + \underset{c}{\mathrm{D}}^2\, 1\tfrac{1}{2} \cdot \mathrm{D}1^{-\frac{3}{2}}\, b^4 + \underset{c}{\mathrm{D}}^3\, 1\tfrac{1}{2} \cdot \underset{c}{\mathrm{D}}^2\, 1^{-\frac{3}{2}} \cdot b^6 + \&c.\right\} \left\{ l.\, \frac{1+\sqrt{1+b}}{\sqrt{b}} - \frac{1}{\sqrt{1+b}} \right\}$$
$$+ \frac{2}{\sqrt{1+b}}$$
$$+ 2\underset{c}{\mathrm{D}}^2\, 1\tfrac{1}{2} \frac{1}{\sqrt{(1+b)}} \cdot \frac{b^3}{2}$$
$$+ \frac{2\underset{c}{\mathrm{D}}^3\, 1\tfrac{1}{2}}{\sqrt{(1+b)}} \left\{\frac{b^4}{4} - \frac{5b^3}{4 \cdot 2}\right\}$$
$$+ \&c.$$

the series $\mathrm{D}1\tfrac{1}{2}\, b^2 + \underset{c}{\mathrm{D}}^2\, 1\tfrac{1}{2} \cdot \mathrm{D}1^{-\frac{3}{2}}\, b^4 + \&c.$ numerically expressed, is
$\frac{1}{2} \cdot b^2 + \frac{1 \cdot 1}{2 \cdot 4} \cdot \frac{3}{2} \cdot b^4 + \frac{1 \cdot 1 \cdot 3}{2 \cdot 4 \cdot 6} \cdot \frac{3 \cdot 5}{2 \cdot 4} b^6 + \&c.$

* This series is the same as
$$-\left\{\mathrm{D}1^{-\frac{1}{2}} b^2 + \mathrm{D}1^{-\frac{1}{2}} \cdot \underset{c}{\mathrm{D}}^2\, 1^{-\frac{1}{2}} b^4 + \underset{c}{\mathrm{D}}^2\, 1^{-\frac{1}{2}} \cdot \underset{c}{\mathrm{D}}^3 1^{-\frac{1}{2}}\, b^6 + \&c.\right\}$$

Let $y = l\,(1+\sqrt{(1+x)})$, then, $dy = \frac{dx}{2\sqrt{(1+x)}\,(1+\sqrt{(1+x)})}$

$$= \frac{dx}{2x} - \frac{dx}{2x\sqrt{(1+x)}}$$

$$= \frac{dx}{2x} - \frac{dx}{2x}\left\{1^{-\frac{1}{2}} - \frac{x}{2} + \frac{1\cdot 3}{2\cdot 4}x^2 - \&c.\right\}$$

$$\therefore y = \frac{x}{4} - \frac{1\cdot 3}{2\cdot 4}\cdot\frac{x^2}{4} + \frac{1\cdot 3\cdot 5}{2\cdot 4\cdot 6}\cdot\frac{x^3}{6} - \&c. + \text{corr.}$$

when $x = 0$ $y = l2 = \therefore$ corr.

hence, $l\,\frac{1+\sqrt{(1+b)}}{\sqrt{b}} - \frac{1}{\sqrt{(1+b)}}$

$$= \frac{b}{4} - \frac{1\cdot 3}{2\cdot 4}\cdot\frac{b^2}{4} + \frac{1\cdot 3\cdot 5}{2\cdot 4\cdot 6}\cdot\frac{b^3}{6} + \&c.$$

$$-\left\{1 - \frac{b}{2} + \frac{1\cdot 3}{2\cdot 4}\cdot b^2 - \frac{1\cdot 3\cdot 5}{2\cdot 4\cdot 6}b^3 + \&c.\right\}$$

$$+ l2 + l\frac{1}{\sqrt{b}}$$

$$= l\frac{2}{\sqrt{b}} - 1 + \frac{3\cdot b}{4} - \frac{3\cdot 5}{2\cdot 4}\cdot\frac{b^2}{4} + \frac{3\cdot 5\cdot 7}{2\cdot 4\cdot 6}\cdot\frac{b^3}{6} - \&c.$$

If this series be substituted for $l\,\frac{1+\sqrt{(1+b)}}{\sqrt{b}} - \frac{1}{\sqrt{(1+b)}}$, in the above form for $f(1)$, if $\frac{1}{\sqrt{(1+b)}}$ be expanded, and the terms affected with like powers of b, be collected, we shall have the same series as LEGENDRE has given. EULER, however, is the original author of the series; and has expressed its law much more clearly than the French mathematician. In the EULERI *Opuscula*, Berlin, 1750, p. 165, the author says, that the elliptic quadrant

$$= 1 + Ab^2 + Bb^4 + C\,b^6 + \&c.$$

$$-\left\{\alpha b^2 + \beta b^4 + \gamma b^6 + \&c.\right\}\ \log.\ b, \text{ in which}$$

$A = \log.\ 2 - \frac{1}{4}$	$\alpha = \frac{1}{2}$
$B = \frac{1\cdot 3}{2\cdot 4}A - \frac{1}{2}(\alpha-\beta) + \frac{1}{2}\cdot\frac{\beta}{2}$	$\beta = \frac{1\cdot 3}{2\cdot 4}\cdot\alpha$
$C = \frac{3\cdot 5}{4\cdot 6}B - \frac{1}{3}(\beta-\gamma) + \frac{1}{4}\cdot\frac{\gamma}{3}$	$\gamma = \frac{3\cdot 5}{4\cdot 6}\beta$
$D = \frac{5\cdot 7}{6\cdot 8}C - \frac{1}{4}(\gamma-\delta) + \frac{1}{6}\cdot\frac{\delta}{4}$	$\delta = \frac{5\cdot 7}{6\cdot 8}\gamma$
$E = \frac{7\cdot 9}{8\cdot 10}D - \frac{1}{5}(\delta-\varepsilon) + \frac{1}{8}\cdot\frac{\varepsilon}{5}$	$\varepsilon = \frac{7\cdot 9}{8\cdot 10}\delta$
&c.	&c.

LEGENDRE's series is easily reducible to this, since log. $\frac{2}{\sqrt{b}} = \frac{1}{2}$ log. $\frac{4}{b} =$ log. $2 - \frac{1}{2}$ log. b.

This memoir of EULER (*Animadversiones in Rectificationem Ellipseos*) is curious, on account of the strange artifices used to obtain the series for the length of the eccentric ellipse. It is characteristical of the peculiar mathematical powers of EULER, and also bears strong marks of the rapidity and eagerness with which he conducted every work of calculation. The author discovers the series and its law, partly by tentative methods, and partly by the use of a differential equation of the second order; and indeed, without the use of such an equation, it is difficult to exhibit the law. Let $f(1)$ represent the whole integral of f, from $x = 0$ to $x = 1$, then,

$$\frac{(1-b^2) \cdot d^2 f(1)}{db^2} - \frac{1+b^2}{b} \cdot \frac{df(1)}{db} + f(1) = 0.$$

Assume then, $f(1) = 1 + Ab^2 + Bb^4 + Cb^6 + \&c.$

$$+\left\{\alpha b^2 + \beta b^4 + \gamma b^6 + \&c.\right\}\log. b;$$

deduce the values of $\frac{d^2 f(1)}{db^2}$, $\frac{df(1)}{db}$; compare the terms affected with like powers of b; and the law of the series, such as it has been exhibited, may be deduced.

The following is the method of deducing the differential equation; $df = dz \int \frac{(1+b^2 z^2)}{(1+z^2)^{\frac{3}{2}}} \therefore \frac{df}{dz} = \frac{\sqrt{(1+b^2 z^2)}}{(1+z^2)^{\frac{3}{2}}}$; and, taking the partial differentials,

$$\frac{d^2 f}{dz.db} = \frac{bz^2}{\sqrt{(1+b^2 z^2)}\,(1+z^2)^{\frac{3}{2}}};$$

consequently, $\frac{df}{dz} = \frac{1}{\sqrt{(1+b^2.z^2)}\,(1+z^2)^{\frac{3}{2}}} + \frac{b\,d^2 f}{dz\,.\,db}$.

and $\frac{d^2 f}{dz\,.\,db} = \frac{-bz^2}{\sqrt{(1+b^2 z^2)^{\frac{3}{2}}}\,(1+z^2)^{\frac{3}{2}}} + \frac{d^2 f}{dz\,.\,db} + \frac{b\,.\,d^3 f}{dz\,.\,db^2}$

$$\therefore \frac{b^2 z^2}{(1+b^2 z^2)^{\frac{3}{2}}\,(1+z^2)^{\frac{3}{2}}} = \frac{b^2 d^3 f}{dz\,.\,db^2};$$

or $\frac{1+b^2z^2}{(1+b^2z^2)^{\frac{3}{2}}(1+z^2)^{\frac{3}{2}}} - \frac{1}{(1+b^2z^2)^{\frac{3}{2}}(1+z^2)^{\frac{3}{2}}} = \frac{b^2d^3f}{dz\,.\,db^2}$,

or $\frac{df}{dz} - \frac{bd^2f}{dz\,.\,db} - \frac{1}{(1+b^2z^2)^{\frac{3}{2}}(1+z^2)^{\frac{3}{2}}} = \frac{b^2d^3f}{dz\,.\,db^2}$.

Now, the differential of $\frac{z}{\sqrt{(1+z^2)(1+b^2z^2)}} = dz\,\frac{(1-b^2z^4)}{(1+z^2)^{\frac{3}{2}}(1+b^2z^2)^{\frac{3}{2}}}$

$\therefore \frac{(b^2-1)\,dz}{b^2(1+z^2)^{\frac{3}{2}}(1+b^2z^2)^{\frac{3}{2}}} = d\left\{\frac{z}{\sqrt{(1+z^2)(1+b^2z^2)}}\right\} + \frac{df}{b^2} + \frac{2dz}{b^2.\overline{1+z^2}^{\frac{3}{2}}\sqrt{1+b^2z^2}}$

$\therefore \int\frac{b^2-1}{b^2}\cdot\frac{dz}{(1+z^2)^{\frac{3}{2}}(1+b^2z^2)^{\frac{3}{2}}} = \frac{z}{\sqrt{(1+z^2)(1+b^2z^2)}} + \frac{f}{b^2} - \frac{2f}{b^2} + \frac{2df}{b\,.\,db}$

$\therefore f - \frac{b\,.\,df}{db} - \frac{b^2}{b^2-1}\,\frac{z}{\sqrt{(1+z^2)(1+b^2z^2)}} + \frac{f}{b^2-1} - \frac{2b}{b^2-1}\,\frac{df}{db} = \frac{b^2d^2f}{db^2}$

$\therefore f - \frac{1+b^2}{b}\cdot\frac{df}{db} + (1+b^2)\,\frac{d^2f}{db^2} = 0$; since, when $x=1$,

$z=\infty$, and $\frac{z}{\sqrt{(1+z^2)(1+b^2z^2)}} = \frac{1}{bz} = 0$.

In order to compute the integral $\int dx\sqrt{\left(\frac{1-e^2x^2}{1-x^2}\right)}\,(f)$, when e is nearly $=1$, by a series ascending by the powers of $\sqrt{(1-e^2)}$, it has been found necessary to establish this formula,

$$\int dx\sqrt{\left(\frac{1-e^2x^2}{1-x^2}\right)} + \int dv\sqrt{\left(\frac{1-e^2v^2}{1-v^2}\right)} = f(1) + \frac{e^2x\sqrt{(1-x^2)}}{\sqrt{(1-e^2x^2)}}.$$

Now, this formula, an analytical artifice useful for computation, applied to a particular curve, and translated into geometrical language, exhibits a curious property of the curve; thus, in an ellipse whose semiaxes are 1, $\sqrt{(1-e^2)}$, $\int dx\sqrt{\left(\frac{1-e^2x^2}{1-x^2}\right)}$, $\int dv\sqrt{\left(\frac{1-e^2v^2}{1-v^2}\right)}$, represent arcs (E, E′) corresponding to abscissas, x, v; and $f(1)$ is the elliptic quadrant (E (1)); hence

$$E + E' = E(1) + e^2x\sqrt{\left(\frac{1-x^2}{1-e^2x^2}\right)}$$

$$\therefore E - \left\{E(1) - E'\right\} = e^2x\sqrt{\left(\frac{1-x^2}{1-e^2x^2}\right)};$$

or the difference of two arcs, one reckoned from the extremity of the conjugate, the other from the extremity of the transverse, is equal to a right line, represented by $e^2x\sqrt{\left(\frac{1\quad x^2}{1-e^2x^2}\right)}$.

This theorem is known by the name of FAGNANI's theorem.* When $x = v$, or the quantity $e^2 x \sqrt{\left(\frac{1-x^2}{1-e^2 x^2}\right)}$ is at its maximum, $2f = f(1) + 1 - b$,

or $f - (f(1) - f) = 1 - b$,

or $E - \{E(1) - E'\} = 1 - b$;

or the elliptic quadrant is divided in such a manner, that the difference of the two arcs = difference of the semiaxes.

From the preceding analysis it is clear, that the computation of the integral of $dx \sqrt{\left(\frac{1-e^2 x^2}{1-x^2}\right)}$ is perfectly independent of the existence of the ellipse and its properties. But it also appears, that the property of the bisection of the ellipse, established geometrically, ought not to be regarded as a merely curious and beautiful property, since, by its aid, the length of the elliptic quadrant may be computed. Several other properties, considered hitherto in the light of curious and speculative truths, translated, would appear analytical artifices, and in computation practically useful.

By the preceding series, the integral of $dx \sqrt{\left(\frac{1-e^2 x^2}{1-x^2}\right)}$ may be computed, when e is nearly $= 0$ or 1. It is necessary, however, to possess a method of computing the integral when e is of mean value; and the methods I am about to exhibit, are such as to supersede the use of the two series ascending by the powers of e and b; in other words, from two similar methods, in all values of e between 0 and 1, the integral may be commodiously computed. The principle of the method is this, if $df = dx \sqrt{\left(\frac{1-e^2 x^2}{1-x^2}\right)}$; then,

* This theorem of FAGNANI has lately been very neatly demonstrated, by a most skilful mathematician, Mr. BRINKLEY, in the Irish Transactions, by a geometrical process, but not without the use of prime and ultimate ratios. Indeed, the nature of the subject is such, that the theorem cannot be established, without the use of the fluxionary calculus, or of some calculus equivalent to it.

df', df'', df''', &c. being similar differential expressions, df may be resolved into $m dP + \alpha . df' + \beta . df''$, ($dP$ being a perfect differential, m, α, β, &c. constant coefficients,) in like manner,

df' may be resolved into $m' dP' + \alpha' . df'' + \beta' . df'''$,

df'' into - - $m'' dP'' + \alpha'' . df''' + \beta'' . df^{\text{iv}}$,

&c.

and, consequently, df may be resolved into

$$m . dP + \alpha m' . dP' + \alpha \alpha' m' m'' dP'' + \&c.$$
$$+ \alpha \alpha' \alpha'' \alpha''' \&c. df''^{\&c.} + \&c.$$

This resolution depends on a very simple, and, if I may use the term, natural substitution, in the form $dx \sqrt{\left(\frac{1 - e^2 x^2}{1 - x^2}\right)}$, of which, to the best of my knowledge, M. LAGRANGE is the author.

Let $y = x \sqrt{\left(\frac{1 - x^2}{1 - e^2 x^2}\right)}$;

then, $x^2 = \frac{e^2 y^2 + 1}{2} - \frac{1}{2} \sqrt{(1 + 2 (e^2 - 2) y^2 + e^4 y^4)}$,

and $\frac{dx}{\sqrt{(1 - x^2)(1 - e^2 x^2)}} = \frac{dy}{1 - 2x^2 + e^2 y^2} = \frac{dy}{\sqrt{(1 + 2 . (e^2 - 2) y^2 + e^4 y^4)}}$.

Now, if $p = 1 + \sqrt{(1 - e^2)}$, $p^2 = 2 - e^2 + 2\sqrt{(1 - e^2)}$,

if $q = 1 - \sqrt{(1 - e^2)}$, $q^2 = 2 - e^2 - 2\sqrt{(1 - e^2)}$,

and $1 + 2 . (e^2 - 2) y^2 + e^4 y^4 = (1 - p^2 y^2) . (1 - q^2 y^2)$

$\therefore \frac{dx}{\sqrt{(1 - x^2)(1 - e^2 x^2)}} = \frac{dy}{\sqrt{(1 - p^2 y^2)(1 - q^2 y^2)}} = \frac{du'}{p . \sqrt{(1 - u'^2)\left(1 - \frac{q^2}{p^2} u'^2\right)}}$;

putting $y = \frac{u'}{p}$, and putting $\frac{q}{p} = e'$,

$\frac{dx}{\sqrt{(1 - x^2)(1 - e^2 x^2)}}$ is transformed into $\frac{du'}{p\sqrt{(1 - u'^2)(1 - e'^2 u'^2)}}$;

similarly, putting $p' = 1 + \sqrt{(1 - e'^2)}$, $e'' = \frac{q'}{p'} = \frac{1 - \sqrt{(1 - e'^2)}}{1 + \sqrt{(1 - e'^2)}}$,

and $u'' = p' u' \sqrt{\left(\frac{1 - u'^2}{1 - e'^2 u'^2}\right)}$,

$\frac{du'}{\sqrt{(1 - u'^2)(1 - e'^2 u'^2)}} = \frac{1}{p'} . \frac{du''}{\sqrt{(1 - u''^2)(1 - e''^2 u''^2)}}$.

Hence, since $e' = \frac{1 - \sqrt{(1 - e^2)}}{1 + \sqrt{(1 - e^2)}}$, $p = 1 + \sqrt{(1 - e^2)} = \frac{2}{1 + e'}$,

$\frac{dx}{\sqrt{(1-x^2)(1-e^2x^2)}}$ may be transformed into

$\frac{1+e'}{2} \cdot \frac{du'}{\sqrt{(1-u'^2)(1-e'^2u'^2)}}$, or $\frac{(1+e')(1+e'')}{2\ .\ 2} \cdot \frac{du''}{\sqrt{(1-u''^2)(1-e''^2u''^2)}}$;

or into $\frac{(1+e')(1+e'')(1+e''') \ \ldots\ 1+e^{(n)}}{2\ .\ 2\ .\ 2\ \ldots\ 2} \cdot \frac{du^{(n)}}{\sqrt{(1-u^{(n)2})(1-e^{(n)2}u^{(n)2})}}$.

And, similarly, putting $U'=\sqrt{(1-u'^2)(1-e'^2u'^2,)}$

$$U''=\sqrt{(1-u''^2)(1-e''^2u''^2)} \text{ \&c.}$$

$\frac{(A+Bx^2)\,.\,dx}{\sqrt{(1-x^2)(1-e^2x^2)}}$, may be transformed into

$$\left(\frac{2A+B}{2p}\right) . \frac{du'}{U'} - \frac{B}{2p} . du' + \frac{Be^2}{2p^3} . \frac{u'^2 . du'}{U'};$$

or, to render the last term like the original form, into

$$\left(\frac{2A+B}{2p} - \frac{Be^2 A'}{2p^3 B'}\right) \frac{du'}{U'} - \frac{B}{2p} du' + \frac{Be^2}{2p^3 B'} (A'+B'u'^2) \frac{du'}{U'}.$$

And, into a form exactly similar may $(A'+B'u'^2) . \frac{du'}{\sqrt{(1-u'^2)(1-e'^2u'^2)}}$ be transformed.

Hence, to transform df or $dx\sqrt{\left(\frac{1-e^2x^2}{1-x^2}\right)} = \frac{dx\,(1-e^2x^2)}{\sqrt{(1-x^2)(1-e^2x^2)}}$

$A=1, B=-e^2$; consequently,

$$df = \frac{e^2}{2p} . du' - \frac{\sqrt{(1-e^2)}}{p} . \frac{du'}{U'} + \frac{p}{2}(1-e'^2u'^2) . \frac{du'}{U'};$$

or, since $\sqrt{(1-e^2)} = \frac{1-e'}{1+e'}$ and $\frac{p}{2} = \frac{1}{1+e'}$

$$df = \frac{e^2}{4}(1+e')\,du' - \frac{1-e'}{2} . \frac{du'}{U'} + \frac{1}{1+e'} . df'; \qquad (a)$$

similarly, $df' = \frac{e'^2}{4} . (1+e'')\,du'' - \left(\frac{1-e''}{2}\right) \frac{du''}{U''} + \frac{1}{1+e''} . df''$;

$$df'' = \frac{e''^2}{4}(1+e''')\,du''' - \text{\&c.}$$

The utility of this transformation will appear, by observing that the quantities e', e'', e''', &c. continually decrease; thus,

$$e' = \frac{1-\sqrt{(1-e^2)}}{1+\sqrt{(1-e^2)}} = \frac{e^2}{(1+\sqrt{(1-e^2)})^2} = e . \frac{e}{(1+\sqrt{(1-e^2)})^2}.$$

Hence, if e be a fraction, $e' = e \times$ a fraction; consequently, e' is $\angle\ e$; similarly, e'' is $\angle\ e'$, $e''' \angle\ e''$ &c; hence, if the series for $\int dx\sqrt{\left(\frac{1-e^2x^2}{1-x^2}\right)}$ does not converge quickly, transform df as above,

and the series for $\frac{du'}{U'}$, df', converge more quickly; but, if not with sufficient rapidity, again transform $\frac{du'}{U'}$, df', and the resulting forms $\frac{du''}{U''}$, df'', may be converted into series of still greater convergency; so that, by this method, we may proceed with certainty to the computation of $\int dx \surd\left(\frac{1-e^2x^2}{1-x^2}\right)$. If we stop at the first transformation, there results a series for f, the same as is given in a very able memoir of Mr. IVORY's, inserted in the Edinburgh Transactions, Vol. IV. p. 178.

Thus, $df = \frac{e^2}{4}(1+e')\,du' - \frac{(1-e')}{2}\cdot\frac{du'}{U'} + \frac{1}{1+e'}\cdot df'$, (a)

$$\text{or} = \frac{e^2}{4}\cdot(1+e')\,du' + \frac{1+e'^2}{2\cdot(1+e')}(A)\frac{du'}{U'} - \frac{e'^2}{1+e'}(B)\cdot\frac{u'^2\,du'}{U'}.$$

Now, $u' = px\surd\left(\frac{1-x^2}{1-e^2x^2}\right)$, which quantity is at its maximum when $x = \frac{1}{\surd(1+b)}$, and then $u' = 1$; consequently, whilst x from 0 becomes 1, u' from 0 passes through its maximum (1) to 0 again; consequently, $\int\frac{du'}{U'}$ from $x=0$ to $x=1$, $= 2\int\frac{du'}{U'}$ from $u'=0$ to $u'=1$.

Now, $\int A\cdot\frac{du'}{U'} - \int B\cdot\frac{u'^2\,du'}{U'}$

$$= \int\frac{A\,du'}{\surd(1-u'^2)}\left\{1^{-\frac{1}{2}} - D1^{-\frac{1}{2}}\cdot e'^2u'^2 + \underset{c}{D}^2\,1^{-\frac{1}{2}}\cdot e'^4u'^4 - \&c.\right\}$$

$$-\int\frac{Bu'^2\cdot du'}{\surd(1-u'^2)}\left\{1^{-\frac{1}{2}} - D1^{-\frac{1}{2}}e'^2u'^2 + \underset{c}{D}^2\,1^{-\frac{1}{2}}\cdot e'^4u'^4 - \&c.\right\}$$

But, by a preceding form, page 227,

$$\int\frac{u'^{2n}\cdot du'}{\surd(1-u'^2)} = -\surd(1-u'^2)\left\{\frac{u'^{2n-1}}{2n} + \frac{2n-1\cdot u'^{2n-3}}{2n(2n-2)} - \&c.\right\} + \frac{(2n-1)(2n-3)\ldots 5\cdot 3}{2n\cdot(2n-2)\ldots 4\cdot 2}\int\frac{du'}{\surd 1-u'^2};$$

put $u'=1$, and all the terms vanish, except the last; consequently, from $u'=0$ to $u'=1$,

$$\int \pm\underset{c}{D}^n\,1^{-\frac{1}{2}}e'^{2n}\cdot\frac{u'^{2n}\,du'}{\surd(1-u'^2)} = \pm\underset{c}{D}^n\,1^{-\frac{1}{2}}\cdot e'^{2n}\times\frac{(2n-1)(2n-3)\ldots 5\cdot 3\cdot 1}{2n\cdot(2n-2)\ldots 6\cdot 4\cdot 2}\cdot\frac{\pi}{2}$$

$\left(\frac{\pi}{2} = \frac{3\cdot 14159+}{2}\right) = \pm \underset{c}{D}^n 1^{-\frac{1}{2}} \times \pm \underset{c}{D}^n 1^{-\frac{1}{2}} . e'^{2n} . \frac{\pi}{2} = \therefore$, whether n be even or odd, $\left(\underset{c}{D}^n 1^{-\frac{1}{2}}\right)^2 e'^{2n} . \frac{\pi}{2}$;

similarly, $\int \pm \underset{c}{D}^n 1^{-\frac{1}{2}} . e'^{2n} . u'^{2n+2} . \frac{du'}{\sqrt{(1-u'^2)}} = -\underset{c}{D}^n 1^{-\frac{1}{2}} . \underset{c}{D}^{n+1} 1^{-\frac{1}{2}} e'^{2n} . \frac{\pi}{2}$

Hence, putting for n the several values 0, 1, 2, 3, 4, &c. the sum of the integrals from $u' = 0$ to $u' = 1$

$$= \frac{A\pi}{2}\left\{1^{-\frac{1}{2}} + \left(D 1^{-\frac{1}{2}}\right)^2 e'^2 + \left(\underset{c}{D}^2 1^{-\frac{1}{2}}\right)^2 e'^4 + \&c.\right\}$$

$$+ \frac{B\pi}{2}\left\{D 1^{-\frac{1}{2}} + D 1^{-\frac{1}{2}} . \underset{c}{D}^2 1^{-\frac{1}{2}} e'^2 + \underset{c}{D}^2 1^{-\frac{1}{2}} . \underset{c}{D}^3 1^{-\frac{1}{2}} e'^4 + \&c.\right\}$$

or, putting for A and B, their values $\frac{1+e'^2}{2.(1+e')}$, $\frac{2e'^2}{2.(1+e')}$, the integral

$$= \frac{\pi}{2.2.(1+e')}\left\{1 + \begin{array}{c} 1^{-\frac{1}{2}} \\ 2D 1^{-\frac{1}{2}} \\ +\left(D 1^{-\frac{1}{2}}\right)^2 \end{array}\right| e'^2 \begin{array}{c} \left(D 1^{-\frac{1}{2}}\right)^2 \\ + 2D 1^{-\frac{1}{2}} . \underset{c}{D}^2 1^{-\frac{1}{2}} \\ \left(\underset{c}{D}^2 1^{-\frac{1}{2}}\right)^2 \end{array}\left| e'^4 + \&c.\right.$$

and, generally, the coefficient affected with e'^{2n} is

$$\left(\underset{c}{D}^{n-1} 1^{-\frac{1}{2}}\right)^2 + 2\underset{c}{D}^{n-1} 1^{-\frac{1}{2}} . \underset{c}{D}^n 1^{-\frac{1}{2}} + \left(\underset{c}{D}^n 1^{-\frac{1}{2}}\right)^2 = \left\{\underset{c}{D}^{n-1} 1^{-\frac{1}{2}} + \underset{c}{D}^n 1^{-\frac{1}{2}}\right\}^2,$$

but $-(2n-1)\,\underset{c}{D}^n 1^{\frac{1}{2}} = \underset{c}{D}^n 1^{-\frac{1}{2}}$,

and $2n\,.\,\underset{c}{D}^n 1^{\frac{1}{2}} = \underset{c}{D}^{n-1} 1^{-\frac{1}{2}}$

$$\therefore \underset{c}{D}^n 1 = \underset{c}{D}^{n-1} 1^{-\frac{1}{2}} + \underset{c}{D}^n 1^{-\frac{1}{2}}.$$

Hence, the coefficient affected with e'^{2n} is $\left(\underset{c}{D}^n 1^{\frac{1}{2}}\right)^2$; and, consequently, the integral from $u' = 0$ to $u' = 1$

$$= \frac{\pi}{2.2.\overline{1+e'}}\left\{1 + \left(D 1^{\frac{1}{2}}\right)^2 . e'^2 + \left(\underset{c}{D}^2 1^{\frac{1}{2}}\right)^2 . e'^4 + \left(\underset{c}{D}^3 1^{\frac{1}{2}}\right)^2 . e'^6 + \&c.\right\}$$

the double of this is the integral (f) from $x = 0$ to $x = 1$,

or $\int dx \sqrt{\left(\frac{1-e^2x^2}{1-x^2}\right)}$ from $x = 0$ to $x = 1$

$$= \frac{\pi}{2(1+e')}\left\{1 + \left(D 1^{\frac{1}{2}}\right)^2 . e'^2 + \left(\underset{c}{D}^2 1^{\frac{1}{2}}\right)^2 . e'^4 + \left(\underset{c}{D}^3 1^{\frac{1}{2}}\right)^2 . e'^6 + \&c.\right\}$$

or, developing the symbols $D 1^{\frac{1}{2}}$ &c.

the integral

$$= \frac{\pi}{2 \cdot (1+e')} \left\{ 1 + \frac{1^2}{2^2} \cdot e'^2 + \frac{1^2 \cdot 1^2}{2^2 \cdot 4^2} \cdot e'^4 + \frac{1^2 \cdot 1^2 \cdot 3^2}{2^2 \cdot 4^2 \cdot 6^2} e'^6 + \&c. \right\}. \qquad (4)$$

Which is the same series as is given in the Edinburgh Transactions, Vol. IV. p. 178, and which its ingenious author, Mr. IVORY, derived from a method of LAGRANGE, contained in the Berlin Acts for 1784. According to that method, $dx \sqrt{\left(\frac{1-e^2 x^2}{1-x^2}\right)}$ is put under the form $A d\theta \left\{ 1 + a^2 + 2a \cos. 2\theta \right\}^{\frac{1}{2}}$, and its exponential expression substituted for cos. 2θ.

I have deduced the preceding series ascending by the powers of e' or of $\frac{1-b}{1+b}$, in order to show, that it is a particular result of the general method of the transformation of $dx \sqrt{\left(\frac{1-e^2 x^2}{1-x^2}\right)}$. For purposes of computation, it will be convenient to push the transformation farther; if, for instance, to quantities involving e'', the integral of $dx \sqrt{\left(\frac{1-e^2 x^2}{1-x^2}\right)}$ from $x = 0$ to $x = 1$, may be computed from 2 series; or the whole integral equals

$$\frac{\pi}{(1+e')(1+e'')} \left\{ 1^{\frac{1}{2}} + (D 1^{\frac{1}{2}})^2 \cdot e''^2 + (\underset{c}{D^2} 1^{\frac{1}{2}})^2 e''^4 + \&c. \right\}$$
$$- \frac{2\pi \cdot (1-e')(1+e'')}{4} \left\{ 1^{-\frac{1}{2}} + (D 1^{-\frac{1}{2}})^2 e''^2 + (D'' 1^{-\frac{1}{2}})^2 e''^4 + \&c. \right\}$$

which expression may be derived after a manner precisely similar to that by which I have deduced the series ascending by the powers of e'.

If the transformation of df be indefinitely continued, there results a form very convenient for the computation of the integral of $dx \sqrt{\left(\frac{1-e^2 x^2}{1-x^2}\right)}$, in all values of e between 0 and $\sqrt{\frac{1}{2}}$; thus,

$$df = \frac{e'}{1+e'} \cdot du' - \frac{1-e'}{2} \cdot \frac{du'}{U'} + \frac{df'}{1+e'}, \qquad (a)$$

$$\text{or} = \frac{e'}{1+e'} \cdot du' + \left\{ \frac{1+e'}{2} - 1 \right\} \frac{du'}{U'} + \frac{df'}{1+e'};$$

similarly, $df' = \frac{e''}{1+e''} \cdot du'' + \left\{ \frac{1+e''}{2} - 1 \right\} \cdot \frac{du''}{U''} + \frac{df''}{1+e''}$.

Hence,

$$df = \frac{e'}{1+e'} \cdot du' + \frac{e''}{(1+e')(1+e'')} \cdot du''$$

$$+ \frac{1+e'}{2} \cdot \frac{du'}{U'} + \frac{1+e''}{2} \cdot \frac{1}{1+e'} \cdot \frac{du''}{U''}$$

$$-\left\{ \frac{du'}{U'} + \frac{1}{1+e'} \cdot \frac{du''}{U''} \right\} + \frac{1}{(1+e')(1+e'')} df''$$

$$= \frac{e'}{1+e'} \cdot du' + \frac{e''}{(1+e')(1+e'')} \cdot du'' + \frac{e'''}{(1+e')(1+e'')(1+e''')} du''' + \&c.$$

$$+ \frac{1+e'}{2} \cdot \frac{du'}{U'} + \frac{1+e''}{2} \cdot \frac{1}{1+e'} \cdot \frac{du''}{U''} + \frac{1+e'''}{2\ (1+e')\ 1+e''} \cdot \frac{du'''}{U'''} + \&c.$$

$$-\left\{ \frac{du'}{U'} + \frac{1}{1+e'} \cdot \frac{du''}{U''} + \frac{1}{(1+e')(1+e'')} \cdot \frac{du'''}{U'''} + \&c. \right\}$$

$$+ \frac{1}{(1+e')(1+e'') \ldots \&c.}, df^{(n)}.$$

Now, $\frac{du'}{U'} = \frac{1+e''}{2} \cdot \frac{du''}{U''} = \frac{(1+e'')(1+e''')}{2\ .\ 2} \cdot \frac{du'''}{U'''}$

$= \frac{(1+e'')(1+e''') \ldots\ldots (1+\varepsilon)}{2^n} \cdot \frac{dv}{V}$, ($\varepsilon$, v, V, representing the last terms of series, e', e'', e''', &c; u', u'', u''', &c; U′, U″, U‴, &c.),

$$\frac{\varepsilon}{(1+e')(1+e'') \ldots 1+\varepsilon} = \frac{e^2 .\ e'\ e''.\ \&c.\ (1+e')(1+e'') \ldots (1+\varepsilon)}{4 \cdot 4 \cdot 4 \cdot \&c.},$$

and, $df^{(n)} = \frac{dv}{V} - \frac{\varepsilon^2 v^2 dv}{V}$; let $P = (1+e')(1+e'') \ldots (1+\varepsilon)$; then,

$$df = \frac{e^2}{4} \cdot (1+e') . du' + \frac{e^2 .\ e'(1+e') . (1+e'')}{4 \cdot 4} \cdot du'' + \frac{e^2 . e' . e'' . (1+e')(1+e'')(1+e''')}{4 \cdot 4 \cdot 4}$$

$$+ \&c.\ (d\,\Sigma u')$$

$$+ \frac{P}{2^n}\left\{ 1 + \frac{2}{(1+e')^2} + \frac{2^2}{(1+e')^2(1+e'')^2} + \frac{2^n}{P^2} + \&c. \right\} . \frac{dv}{V}$$

$$- \frac{P}{2^n}\left\{ 0 + \frac{2\,.}{1+e'} + \frac{2^2}{(1+e')^2(1+e'')} + \frac{2^n\,(1+e)}{P^2} + \&c. \right\} + \frac{\varepsilon^2\,dv}{2^n\,P\,.\,V};$$

consequently,

$$df = d\,\Sigma u' + \frac{P}{2^n} \cdot \frac{dv}{V}$$

$$- \frac{P}{2^n}\left\{ \frac{2e'}{(1+e')^2} + \frac{2^2\,e''}{(1+e')^2(1+e'')^2} + \&c. \right\} . \frac{dv}{V}$$

$$+ \frac{\varepsilon^2}{P} \cdot \frac{dv}{V};$$

and, consequently, since $\frac{2e'}{(1+e')^2} = \frac{e^2}{2}$, $\frac{2^2\,e''}{(1+e')^2(1+e'')^2} = \frac{e^2\ e'}{2\ .\ 2}$ &c.

(for e' being $= \frac{1-\sqrt{(1-e^2)}}{1-\sqrt{(1-e^2)}}$, $e^2 = \frac{4e'}{(1+e')^2}$),

$$f = \Sigma u' + \frac{P}{2^n} \int \frac{dv}{V}$$
$$- \frac{P}{2^n} \cdot \left\{ \frac{e^2}{2} + \frac{e^2 . e'}{2 . 2} + \frac{e^2 . e' . e''}{2 . 2 . 2} + \&c. \right\} \int \frac{dv}{V}$$
$$+ \frac{\varepsilon^2}{P} \int \frac{dv}{V}.$$

Now, $\frac{\varepsilon^2}{P} = \frac{e^2 . e' . e'' . \&c. \ldots . \varepsilon}{4 . 4 . 4 \quad \ldots . 4} . (1+e')(1+e'')(1+e''') \ldots (1+\varepsilon)$; consequently, since e', e'', e''', &c. continually decrease, the quantity $\frac{\varepsilon^2}{P} \int \frac{dv}{V}$ may be rejected, and $\frac{dv}{V} = \frac{dv}{\sqrt{(1-V^2)(1-e^2 V^2)}}$, nearly $= \frac{dv}{\sqrt{(1-V^2)}}$; consequently,

$$f = \frac{e^2}{4}(1+e') u' + \frac{e^2 . e' . (1+e')(1+e'')}{4 . 4} u'' + \&c. \tag{5}$$
$$+ \frac{P}{2^n} \int \frac{dv}{\sqrt{(1-V^2)}} - \frac{P}{2^n} \left\{ \frac{e^2}{2} + \frac{e^2 . e'}{2 . 2} + \frac{e^2 . e' . e''}{2 . 2 . 2} + \&c. \right\} \int \frac{dv}{\sqrt{(1-V^2)}}.$$

When x passes from 0 to 1, u' passes from 0 to 1, (its maximum,) and from 1 to 0; similarly, when u' passes from 0 to 1, u'' passes from 0 to 1, (its maximum,) and from 1 to 0. Hence, $\int \frac{dx}{\sqrt{(1-x^2)}}$ generated from $x = 0$ to $x = 1 = 2 \int \frac{du'}{\sqrt{(1-u'^2)}}$, from $u' = 0$ to $u' = 1$; $= 4 \int \frac{du''}{\sqrt{(1-u''^2)}}$, from $u'' = 0$ to $u'' = 1$; $= 2^n \int \frac{dv}{\sqrt{(1-V^2)}}$, from $v = 0$ to $v = 1$; consequently, since u', u'', u''', &c. $= 0$, when $x = 1$, the whole integral of $dx \sqrt{\left(\frac{1-e^2 x^2}{1-x^2}\right)}$ from $x = 0$ to $x = 1$

$$= \frac{P\, 2^n}{2^n} . \frac{\pi}{2} - \frac{P}{2^n} \left\{ \frac{e^2}{2} + \frac{e^2 . e'}{2 . 2} + \frac{e^2 . e' . e''}{2 . 2 . 2} + \&c. \right\} 2^n . \frac{\pi}{2}$$
$$= (\text{putting } Q = \frac{e}{2} + \frac{e . e'}{2 . 2} + \frac{e . e' . e''}{2 . 2 . 2} + \&c.)\ P . \frac{\pi}{2} - P e Q \frac{\pi}{2}$$
$$= P(1 - eQ) \frac{\pi}{2}. \tag{6}$$

Which is the same form as was first given by Mr. WALLACE, in the Edinburgh Transactions, Vol. V. p. 280.

The form (5) may easily be made to agree with that given, by the last mentioned author, for the length of an elliptic arc.

Thus, $u' = \frac{2}{1+e'} . x \sqrt{\left(\frac{1-x^2}{1-e^2 x^2}\right)}$, $u'' = \frac{2}{1+e''}$, $u' \sqrt{\left(\frac{1-u'^2}{1-e'^2 u'^2}\right)}$ &c.

If we call, then, x the sine of an arc θ,

$$u' = \frac{2}{1+e'} \cdot \frac{\sin. \theta . \cos. \theta}{\sqrt{(1-e^2 . \sin. \theta^2)}} = \frac{\sin. 2\theta}{(1+e') \sqrt{(1-e^2(\frac{1}{2}-\frac{1}{2}\cos. 2\theta))}}$$

$= \left(\text{since } \frac{e^2}{2} = \frac{2e'}{(1+e')^2}\right) \frac{\sin. 2\theta}{\sqrt{(1+e'^2+2e' . \cos. 2\theta)}}$; similarly, calling u' the sine of 2θ, u'' will equal $\frac{\sin. 4\theta'}{\sqrt{(1+e''^2+2e'' . \cos. 4\theta)}}$, and so on; consequently, expressed in geometrical language,

$$f = \frac{e^2}{4}(1+e') \sin. 2\theta' + \frac{e^2 . e'(1+e')(1+e'')}{4 \cdot 4} . \sin. 4\theta'' + \&c.$$

$+ P . \phi - PeQ\phi$, (where ϕ is the limit to which the arcs in the series θ', θ'', θ''', &c. approach,) and, consequently, since $v = 2^n \phi$, $\frac{dv}{\sqrt{(1-V^2)}} = 2^n . d\phi$, and $\frac{P}{2^n} . \int \frac{dv}{\sqrt{(1-V^2)}} = P\phi$.

Mr. Wallace obtained his formula, following a method given by Mr. Ivory, in the fourth Volume of the Edinburgh Transactions; and both these ingenious authors have employed, probably without adopting, the substitution of Lagrange, and the principle of his transformation, such as that great mathematician uses in finding the integral of $\frac{P . dx}{\sqrt{(e+fx^2)(g+hx^2)}}$.

Since $e' = \frac{e^2}{(1+\sqrt{(1-e^2)})^2}$, $e'' = \frac{e'^2}{(1+\sqrt{(1-e'^2)})^2}$ &c.

When e is a small fraction, the quantities e', e'', e''', &c. decrease very rapidly; and, consequently, the preceding form is very commodious for the computation of $\int dx \sqrt{\left(\frac{1-e^2 x^2}{1-x^2}\right)}$, when e is any fraction between 0 and $\sqrt{\frac{1}{2}}$. It ceases, however, to be commodious when e is nearly $= 1$, or is not equally commodious with the series $1 + Ab^2 + Bb^4 + \&c.$

$+ \left\{\alpha b^2 + \beta b^4 + \&c.\right\} \log. b$, given page 234. I purpose, therefore, now to exhibit a form by which the integral of $dx \sqrt{\left(\frac{1-e^2 x^2}{1-x^2}\right)}$ may be conveniently computed, when e is any fraction between $\sqrt{\frac{1}{2}}$ and 1,

$df = dx\sqrt{\left(\frac{1-e^2x^2}{1-x^2}\right)}$. Let $x = \frac{v}{\sqrt{(1+v^2)}}$, then, $df = \frac{dv\sqrt{(1+b^2v^2)}}{(1+v^2)^{\frac{3}{2}}}$
$= d\left(\frac{v}{\sqrt{(1+v^2)}}\sqrt{(1+b^2v^2)}\right) - \frac{b^2v^2dv}{\sqrt{(1+v^2)(1+b^2v^2)}}$.
Let $z = v\sqrt{\left(\frac{1+v^2}{1+b^2v^2}\right)}$, then $v^2 = \frac{b^2z^2 - 1 + \sqrt{(1+p^2z^2)(1+q^2z^2)}}{2}$;
since $(1+p^2z^2)(1+q^2z^2) = 1 + 2.(2-b^2)z^2 + b^4z^4$, when $p = 1+\sqrt{(1-b^2)}$ and $q = 1-\sqrt{(1-b^2)}$.
Hence, since $\frac{dv}{\sqrt{(1+v^2)(1+b^2v^2)}} = \frac{dz}{\sqrt{(1+p^2z^2)(1+q^2z^2)}}$
$= \frac{dz'}{p\sqrt{(1+z'^2)(1+{}^{\backprime}b^2z'^2)}\,(Z')}$, (putting $z = \frac{z'}{p}$, ${}^{\backprime}b = \frac{q}{p} = \frac{1-\sqrt{(1-b^2)}}{1+\sqrt{(1-b^2)}}$),
we have

$$\frac{b^2v^2dv}{\sqrt{(1+v^2)(1+b^2v^2)}} = \frac{b^2dz'}{p\,.\,Z'}\cdot\left\{\frac{b^2z'^2}{2p^2} - \tfrac{1}{2} + \tfrac{1}{2}Z'\right\}$$

$$= \frac{b^2}{4}\cdot(1+{}^{\backprime}b)\,dz' - \frac{b^2}{4}(1+{}^{\backprime}b)\frac{dz'}{Z'} + \frac{{}^{\backprime}b^2}{1+{}^{\backprime}b}\cdot\frac{z'^2dz'}{Z'}.$$

Similarly, putting $z'' = {}^{\backprime}p\frac{z'\sqrt{1+z'^2)}}{\sqrt{1+{}^{\backprime}b^2z'^2)}}$, ${}^{\backprime\backprime}b = \frac{1-\sqrt{(1-{}^{\backprime}b^2)}}{1+\sqrt{(1-{}^{\backprime}b^2)}}$,
$\sqrt{(1+z''^2)(1+{}^{\backprime\backprime}b^2z''^2)} = Z''$,

$$\frac{{}^{\backprime}b^2z'^2.\,dz'}{Z'} = \frac{{}^{\backprime}b^2}{4}(1+{}^{\backprime\backprime}b)\,dz'' - \frac{{}^{\backprime}b^2}{4}(1+{}^{\backprime\backprime}b)\frac{dz''}{Z''} + \frac{{}^{\backprime\backprime}b^2}{1+{}^{\backprime\backprime}b}\cdot\frac{z''^2dz''}{Z''},$$

&c. &c.

Consequently, since $\frac{dz'}{Z'} = \frac{1+{}^{\backprime\backprime}b}{2}\cdot\frac{dz''}{Z''} = \frac{1+{}^{\backprime\backprime\backprime}b\,.\,1+{}^{\backprime\backprime}b}{2\;.\;2}\cdot\frac{dz'''}{Z'''}$ &c.

$$df = d\left(v\sqrt{\left(\frac{1+b^2v^2}{1+v^2}\right)}\right)$$

$$-\left\{\frac{b^2}{4}(1+{}^{\backprime}b)\,dz' + \frac{{}^{\backprime}b^2}{4}\cdot\frac{1+{}^{\backprime\backprime}b}{1+{}^{\backprime}b}\cdot dz'' + \frac{{}^{\backprime\backprime}b^2}{4}\frac{1+{}^{\backprime\backprime\backprime}b}{(1+{}^{\backprime}b)(1+{}^{\backprime\backprime}b)}.dz''' + \text{\&c.}\right\}$$

$$+\left\{\begin{array}{l} +\frac{b^2}{4}.(1+{}^{\backprime}b).\frac{1+{}^{\backprime\backprime}b}{2}.\frac{1+{}^{\backprime\backprime\backprime}b}{2}\;\ldots\ldots\;\frac{1+\beta}{2} \\ +\frac{{}^{\backprime}b^2}{4(1+{}^{\backprime}b)}.(1+{}^{\backprime\backprime}b).\frac{1+{}^{\backprime\backprime\backprime}b}{2}\;\ldots\ldots\;\frac{1+\beta}{2} \\ +\frac{{}^{\backprime\backprime}b^2}{4.(1+{}^{\backprime}b)(1+{}^{\backprime\backprime}b)}(1+{}^{\backprime\backprime\backprime}b)\;\ldots\ldots\;\frac{1+\beta}{2} \\ +\text{\&c.}\end{array}\right\}\frac{d\zeta}{\sqrt{(1+\zeta^2)(1+\beta^2\zeta^2)}}$$

$$\frac{-\beta^2}{(1+{}^{\backprime}b)(1+{}^{\backprime\backprime}b)\ldots\ldots(1+\beta)},\ \frac{\zeta^2d\zeta}{\sqrt{(1+\zeta^2)(1+\beta^2\zeta^2)}};$$

β, ζ, being the last terms of the series ${}^{\backprime}b$, ${}^{\backprime\backprime}b$, ${}^{\backprime\backprime\backprime}b$, &c. z', z'', z''', &c. continued to n terms; put the product $(1+{}^{\backprime}b)(1+{}^{\backprime\backprime}b)\ldots(1+\beta) = {}^{\backprime}P$,

and $\frac{b}{2} + \frac{b\,.\,{}^{\backprime}b}{2\,.\,2} + \frac{b\,.\,{}^{\backprime}b\,.\,{}^{\backprime\backprime}b}{2\,.\,2\,.\,2} + \text{\&c.} = {}^{\backprime}Q$; then, since $\frac{\zeta^2\,d\zeta}{\surd(1+\zeta^2)\,(1+\beta^2\,\zeta^2)}$ nearly $= \frac{\zeta^2\,d\zeta}{\surd(1+\zeta^2)} = \frac{1}{2}\,d\,(\zeta\surd(1+\zeta^2)) - \frac{1}{2}\,\frac{d\zeta}{\surd(1+\zeta^2)}$,

and $\frac{\beta^2}{2\,.\,{}^{\backprime}P} = \frac{b^2\,.\,{}^{\backprime}b\,.\,{}^{\backprime\backprime}b\,\ldots\ldots\,\beta\,.\,(1+{}^{\backprime}b)\,(1+{}^{\backprime\backprime}b)\,\ldots\,(1+\beta)}{2\,.\,2\,.\,2\quad .\quad 2\quad .\quad 2\quad .\quad 2}$,

we have

$$f = v\surd\left(\frac{1+b^2\,v^2}{1+v^2}\right) \tag{7}$$
$$-\left\{\frac{b^2\,.\,1+{}^{\backprime}b}{2\,.\,2}\,z' + \frac{b^2\,.\,{}^{\backprime}b\,.\,(1+{}^{\backprime}b)\,(1+{}^{\backprime\backprime}b)}{2\,.\,2\,.\,2\,.\,2}\,z'' + \frac{b^2\,.\,{}^{\backprime}b\,.\,{}^{\backprime\backprime}b\,(1+{}^{\backprime}b)\,(1+{}^{\backprime\backprime}b)\,(1+{}^{\backprime\backprime\backprime}b)}{2\,.\,2\,.\,2\,.\,2\,.\,2\,.\,2}\,z''' + \text{\&c.}\right\}$$
$$+\frac{b\,.\,{}^{\backprime}P}{2^n}\left\{\frac{b}{2} + \frac{b\,.\,{}^{\backprime}b}{2\,.\,2} + \frac{b\,.\,{}^{\backprime}b\,.\,{}^{\backprime\backprime}b}{2\,.\,2\,.\,2} + \text{\&c.}\right\}\text{h. log. }\zeta + \surd(1+\zeta^2);$$

since the last term, to wit, $\frac{b^2\,.\,{}^{\backprime}b\,.\,{}^{\backprime\backprime}b\,\ldots\ldots\,\beta\,.\,(1+{}^{\backprime}b)\,(1+{}^{\backprime\backprime}b)\,\ldots\ldots\,(1+\beta)}{2\,.\,2\,.\,2\quad 2\quad .\quad 2\quad 2} \times$ $\zeta + \surd(1+\zeta^2)$, or $\frac{\beta^2}{{}^{\backprime}P}\,.\,\frac{(\zeta+\surd(1+\zeta^2)}{2}$, may be neglected, on account of its smallness.

Suppose it were required to find, from this form, the whole integral of $dx\surd\left(\frac{1-e^2\,x^2}{1-x^2}\right)$ from $x=0$ to $x=1$, put $v = \frac{1}{\surd b}$, then $z' = \frac{2}{1+{}^{\backprime}b}\,.\,v\surd\left(\frac{1+v^2}{1+b^2\,v^2}\right) = \frac{2}{(1+{}^{\backprime}b)\,.\,b} = \left(\text{since } b^2 = \frac{4\,{}^{\backprime}b}{(1+{}^{\backprime}b)^2}\right)\frac{1}{\surd\,{}^{\backprime}b}$; similarly, $z'' = \frac{1}{\surd\,{}^{\backprime\backprime}b}$, $z''' = \frac{1}{\surd\,{}^{\backprime\backprime\backprime}b}$ &c.

Consequently, since

$$f = v\surd\left(\frac{1+b^2\,v^2}{1+v^2}\right)$$
$$-\frac{b^2}{2}\left(\frac{1+{}^{\backprime}b}{2}\,z' + \frac{{}^{\backprime}b\,.\,(1+{}^{\backprime}b)\,(1+{}^{\backprime\backprime}b)}{2\,.\,2\,.\,2}\,z'' + \frac{{}^{\backprime}b\,.\,{}^{\backprime\backprime}b\,.\,(1+{}^{\backprime}b)\,(1+{}^{\backprime\backprime}b)\,(1+{}^{\backprime\backprime\backprime}b)}{2\,.\,2\,.\,2\,.\,2\,.\,2}\,z''' + \text{\&c.}\right.$$
$$+\frac{b\,.\,{}^{\backprime}P\,{}^{\backprime}Q}{2^n}\,.\,l.\left(\zeta+\surd(1+\zeta^2)\right),$$

($l.$ denoting the NAPEREAN logarithm,) when $v = \frac{1}{\surd b}$,

$$f = 1$$
$$-\frac{b}{2}\left(\frac{b\,.\,1+{}^{\backprime}b}{2}\,.\,\frac{1}{\surd\,{}^{\backprime}b} + \frac{b\,{}^{\backprime}b\,(1+{}^{\backprime}b)\,(1+{}^{\backprime\backprime}b)}{2\,.\,2\,.\,2}\,.\,\frac{1}{\surd\,{}^{\backprime\backprime}b} + \text{\&c.}\right\}$$
$$+\frac{b\,.\,{}^{\backprime}P\,{}^{\backprime}Q}{2^n}\,.\,l.\left(\frac{1+\surd(1+\beta)}{\surd\beta}\right)$$

$$= 1$$
$$- \frac{b}{2}\left\{1 + \frac{b \,.\, 1+{}'b}{2 \,.\, 2} + \frac{b \,.\, {}'b \,.\, (1+{}'b)\,(1+{}''b)}{2 \,.\, 2 \,.\, 2 \,.\, 2} + \text{\&c.}\right\}$$
$$+ \frac{b}{2^n} \,.\, {}'\mathrm{P} \,.\, {}'\mathrm{Q} \,.\, l. \left(\frac{1+\sqrt{(1+\beta)}}{\sqrt{\beta}}\right);$$

but it has been shown that, $f(1)$ denoting the integral of $dx\sqrt{\left(\frac{1-e^2 x^2}{1-x^2}\right)}$ when $x = 1, f(1) = 2f\left(x = \frac{1}{\sqrt{(1+b)}}\right) - 1 + b$; consequently,

$$f(1) = 1 \qquad (8)$$
$$- b\left\{\frac{b \,.\, 1+{}'b}{2 \,.\, 2} + \frac{b \,.\, {}'b \,.\, (1+{}'b)\,(1+{}''b)}{2 \,.\, 2 \,.\, 2 \,.\, 2} + \text{\&c.}\right\}$$
$$+ \frac{2b}{2^n} \,.\, {}'\mathrm{P} \,.\, {}'\mathrm{Q} \,.\, l. \left(\frac{1+\sqrt{(1+\beta)}}{\sqrt{\beta}}\right).$$

Since ${}'b = \frac{b^2}{(1+\sqrt{(1-b^2)})^2}$, ${}''b = \frac{{}'b^2}{(1+\sqrt{(1-{}'b^2)})^2}$ &c. the terms ${}'b, {}''b, {}'''b$, &c. decrease very rapidly; and b being a small fraction, ${}'b$ is nearly $= \frac{b^2}{4}$, ${}''b$ more nearly $= \frac{{}'b^2}{4}$, ${}'''b$ more nearly $= \frac{{}''b^2}{4}$, &c. Suppose, then, in the series ${}'b, {}''b, {}'''b$, &c. β, that $\beta_{,}, \beta_{,,}, \beta_{,,,}$, &c. are the terms preceding β, $\beta = \frac{\beta_{,}^2}{4}$, $\beta_{,} = \frac{\beta_{,,}^2}{4}$, &c. consequently, $l. \left(\frac{1+\sqrt{(1+\beta)}}{\sqrt{\beta}}\right) = l. \frac{2}{\sqrt{\beta}} = \frac{1}{2} \,.\, l. \frac{4}{\beta} = \frac{1}{2} \,.\, l. \frac{4^2}{\beta_{,}^2} = 2 \,.\, l. \frac{2}{\sqrt{\beta_{,}}} =$, similarly, $2^2 . l. \frac{2}{\sqrt{\beta_{,,}}} = 2^3 . l. \frac{2}{\sqrt{\beta_{,,,}}}$ &c. Hence, supposing ${}^{(m)}b$ the term in the series ${}'b, {}''b, {}'''b$, &c.* after which, without sensible error, each term $= \frac{1}{4}$ of the square of the preceding term, we have

$$f(1) = 1$$
$$- b\left\{\frac{b \,.\, 1+{}'b}{2 \,.\, 2} + \frac{b \,.\, {}'b \,.\, (1+{}'b)\,(1+{}''b)}{2 \,.\, 2 \,.\, 2 \,.\, 2} + \text{\&c.}\right\} \qquad (9)$$
$$+ \frac{2b}{2^m} \,.\, {}'\mathrm{P} \,.\, {}'\mathrm{Q} \,.\, l. \left(\frac{2}{\sqrt{{}^{(m)}b}}\right),$$

* Or thus, $l. \frac{1+\sqrt{(1+\beta)}}{\sqrt{\beta}} = l. \frac{2}{\sqrt{\beta}} . + \frac{\beta}{4} - \frac{1 \,.\, 3}{2 \,.\, 4} . \frac{\beta^2}{4} + \text{\&c.} = l. \frac{2}{\sqrt{\beta}} + \frac{\beta}{4}$, very nearly $\therefore$ instead of $l. \left(\frac{1+\sqrt{1+\beta}}{\sqrt{\beta}}\right)$, we may put $l. \frac{2}{\sqrt{\beta}} + \frac{\beta}{4}$, or $2 l. \frac{2}{\sqrt{\beta_{,}}} + \frac{\beta_{,}^2}{4^2}$, or $2^2 l. \frac{2}{\sqrt{\beta_{,,}}} + \frac{\beta_{,,}^4}{4^4}$, or $2^3 . l. \frac{2}{\sqrt{\beta_{,,,}}} + \frac{\beta_{,,,}^8}{4^8}$, or $2^{n-m} l. \frac{2}{{}^{(m)}b}$, very nearly.

or $= 1$

$$- b\left\{\frac{b \cdot 1 + {}^{\backprime}b}{2 \cdot 2} + \frac{b \cdot {}^{\backprime}b\,(1+{}^{\backprime}b)\,(1+{}^{\backprime\backprime}b)}{2\;2 \cdot 2 \cdot 2} + \&c.\right\} \qquad (9)$$

$$+ \frac{b}{2^m} \cdot {}^{\backprime}P \cdot {}^{\backprime}Q \cdot l. \frac{4}{{}^{(m)}b},$$

in which, the last term, $\frac{b}{2^m} \cdot {}^{\backprime}P \cdot {}^{\backprime}Q \cdot l. \frac{4}{{}^{(m)}b}$, is, in particular values of m, $\frac{b}{2^2} \cdot {}^{\backprime}P \cdot {}^{\backprime}Q \cdot l. \frac{4}{{}^{\backprime\backprime}b}$, or $\frac{b}{2^3} \cdot {}^{\backprime}P \cdot {}^{\backprime}Q \cdot l. \frac{4}{{}^{\backprime\backprime\backprime}b}$, or $\frac{b}{2^4} \cdot {}^{\backprime}P \cdot {}^{\backprime}Q \cdot l. \frac{4}{{}^{\backprime\backprime\backprime\backprime}b}$ &c. each successive value being nearer the truth.

Let $b = \sqrt{\frac{1}{2}} \therefore \sqrt{(1-b^2)}$ or $e = \sqrt{\frac{1}{2}}$; hence, the two formulas for the integral of $dx\sqrt{\left(\frac{1-e^2x^2}{1-x^2}\right)}$ being equal, and the terms of the series $b, {}^{\backprime}b, {}^{\backprime\backprime}b, {}^{\backprime\backprime\backprime}b$, &c. being respectively equal the terms of the series e, e', e'', e''', &c. we have $P = {}^{\backprime}P$, $Q = {}^{\backprime}Q$, and

$$\frac{P \cdot \pi}{2} \cdot (1 - bQ)$$

$$= 1 - b\left\{\frac{b \cdot 1 + {}^{\backprime}b}{2 \cdot 2} + \frac{b \cdot {}^{\backprime}b \cdot (1+{}^{\backprime}b)\,(1+{}^{\backprime\backprime}b)}{2 \cdot 2 \cdot 2 \cdot 2} + \&c.\right\} + \frac{b}{2^m} \cdot P \cdot Q \cdot l. \frac{4}{{}^{(m)}b}.$$

The two forms (5) (7), are fully adequate to the computation of the integral of $dx\sqrt{\left(\frac{1-e^2x^2}{1-x^2}\right)}$ in all values of e; the series (5), involving e, e', e'', e''', &c. is to be used, when e is any value between 0 and $\sqrt{\frac{1}{2}}$; and the series (7), involving $b \cdot {}^{\backprime}b, {}^{\backprime\backprime}b, {}^{\backprime\backprime\backprime}b$, &c. is to be used, when e is any value between $\sqrt{\frac{1}{2}}$ and 1, or, what is the same thing, when b is any value between 0 and $\sqrt{\frac{1}{2}}$.

From the preceding forms may be deduced a very curious and remarkable theorem for the circumference of a circle, which I shall now exhibit.

By former substitution, $u' = \frac{2}{1+e'}\,x\sqrt{\left(\frac{1-x^2}{1-e^2x^2}\right)}$;

and $\therefore$, when $x = \frac{1}{\sqrt{(1+b)}}$, $u' = 1$.

Hence, $\int\frac{dx}{\sqrt{(1-x^2)\,(1-e^2x^2)}}\left(x = \frac{1}{\sqrt{(1+b)}}\right) = \frac{1+e'}{2}\int\frac{du'}{U'}\,(u'=1)$,

and $\int\frac{dx}{\sqrt{(1-x^2)\,(1-e^2x^2)}}\,(x=1) = \frac{1+e'}{2} \cdot 2\int\frac{du'}{U'}\,(u'=1)$.

Consequently,

$$\int\frac{dx}{\sqrt{(1-x^2)(1-e^2x^2)}}\ (x=1)=2\int\frac{dx}{\sqrt{(1-x^2)(1-e^2x^2)}}\ \left(x=\frac{1}{\sqrt{(1+b)}}\right);$$

but $\int\frac{dx}{\sqrt{(1-x^2)(1-e^2x^2)}}=\frac{(1+e')}{2}\cdot\frac{(1+e'')}{2}\cdots\frac{1+\varepsilon}{2}\int\frac{d\nu}{\sqrt{(1-\nu^2)}}$

$$=(1+e')(1+e'')\ 1+\varepsilon\ .\ \frac{\pi}{2},\text{ when }x=1.$$

Again, $\int\frac{dx}{\sqrt{(1-x^2)(1-e^2x^2)}}=\int\frac{dv}{\sqrt{(1+v^2)(1+b^2v^2)}}$, putting $x=\frac{v}{\sqrt{1+v^2}}$,

and $\int\frac{dv}{\sqrt{(1+v^2)(1+b^2v^2)}}=\frac{1+{}^{\backprime}b}{2}\cdot\frac{1+{}^{\backprime\backprime}b}{2}\cdots\frac{1+\beta}{2}\,l.\,(\zeta+\sqrt{1+\zeta^2})$,

and, when $v=\frac{1}{\sqrt{b}}$, that is, when $x=\frac{1}{\sqrt{(1+b)}}$,

$$\int\frac{dv}{\sqrt{(1+v^2)(1+b^2v^2)}}=\frac{1+{}^{\backprime}b}{2}\cdot\frac{1+{}^{\backprime\backprime}b}{2}\cdots\frac{1+\beta}{2}\,.\,l.\left(\frac{1+\sqrt{(1+\beta)}}{\sqrt{\beta}}\right).$$

Hence, since $\int\frac{dv}{\sqrt{(1+v^2)(1+b^2v^2)}}\left(v=\frac{1}{\sqrt{b}}\right)=\int\frac{dx}{\sqrt{(1-x^2)(1-e^2x^2)}}$.

$\left(x=\frac{1}{\sqrt{(1+b)}}\right)=\frac{1}{2}\int\frac{dx}{\sqrt{(1-x^2)(1-e^2x^2)}}\ (x=1)$,

we have $2\ .\ \left\{\frac{(1+{}^{\backprime}b)}{2}\cdot\frac{(1+{}^{\backprime\backprime}b)}{2}\cdots\frac{1+\beta}{2}\right\}l.\left(\frac{1+\sqrt{(1+\beta)}}{\sqrt{\beta}}\right)$

$=(1+e')(1+e'')\ \ldots\ (1+\varepsilon)\ .\ \frac{\pi}{2}$.

Let now $e=\sqrt{\tfrac{1}{2}}\ \therefore\ b=\sqrt{\tfrac{1}{2}}\ \therefore\ e'={}^{\backprime}b,\ e''={}^{\backprime\backprime}b$ &c. and $\varepsilon=\beta$

$$\therefore\ \frac{2\ .\ l.\ (1+\sqrt{(1+\beta)}}{2^n\ \times\ \sqrt{\beta}}=\frac{\pi}{2};$$

or, from what has preceded,

$$\frac{l.\ \frac{4}{{}^{(m)}b}}{2^m}=\frac{\pi}{2}.$$

In particular cases,

$\frac{\pi}{2}=2^{-3}.\,l.\,\frac{4}{{}^{\backprime\backprime\backprime}b}=$ (more nearly) $2^{-4}.\,l.\,\frac{4}{{}^{\text{iv}}b}$ (more nearly) $2^{-5}.\,l.\,\frac{4}{{}^{\text{v}}b}$ &c.*

* Or thus, when $e=\sqrt{\frac{1}{2}}$. e^{iv} will be a very small fraction, for 10 zeros will precede the first significant figure.

$(1+e^{\text{v}})(1+e^{\text{vi}})(1+e^{\text{vii}})\ \ldots\ (1+\varepsilon)\frac{\pi}{2}=22^{-5}.\,l.\left(\frac{1+\sqrt{1+{}^{\text{v}}b}}{\sqrt{{}^{\text{v}}b}}\right)$,

or $(1+e^{\text{v}})(1+e^{\text{vi}})(1+e^{\text{vii}})\ \ldots\ (1+\varepsilon)\ .\ \frac{\pi}{2}=2^{-4}\ .\ l.\,\frac{4}{{}^{\text{iv}}b}$,

or very nearly $\frac{\pi}{2}=2^{-4}\ .\ l.\,\frac{4}{{}^{\text{iv}}b}$.

It has been shown, (page 239), that $\int \frac{(A+Bx^2)\,dx}{\sqrt{(1-x^2)(1-e^2x^2)}}$ (F) may be transformed into a form such as

$$m \,.\, u' + n\int\frac{du'}{U'} + \alpha . \int\frac{(A'+B'u'^2)\,du'}{\sqrt{(1-u'^2)(1-e'^2u'^2)}}\ (F');$$

and, similarly, F′ into a form as $m'u'' + n'\int\frac{du''}{U''} + \alpha' . F''$.

Consequently, since $\int\frac{du'}{U'} = \frac{1+e''}{2} . \int\frac{du''}{U''}$, we can exterminate $\int\frac{du''}{U''}$, and obtain a resulting equation, such as

$\beta F + \gamma F' + \delta F'' + pu' + qu'' = 0$; which expresses the relation between the integrals of three expressions similar to $dx\sqrt{\left(\frac{1-e^2x^2}{1-x^2}\right)}$.

If $x=1$, then u', u'', u''', &c. $=0$; consequently, $\beta F(1) + 2\gamma F'(1) + 4\delta . F''(1) = 0$, since, x passing from 0 to 1, u' passes from 0 to its maximum (1), and from 1 to 0; consequently, between the values of x, 0, and 1, $\int dF' = 2F'(1)$, $F'(1)$ representing what the integral F′ becomes when $u'=1$; similarly, $\int d\,F''$, when $x=1$, $=4F''(1)$.

Since similar equations must be true for F′, F″, F‴, for F″, F‴, F^{iv}, &c. as for F, F′, F″, it is plain that, by a simple process of elimination, we may arrive at an equation of the form $\beta F + \mu F^{(n-1)} + \nu F^{(n)} + \pi u' + \varrho u'' + \&c. = 0$, β, π, ν, ϱ, &c. being constant quantities, $F^{(n-1)}, F^{(n)}$, the two last terms of the series F′, F″, F‴, &c.

It is clear also, that we can obtain an equation as $\beta F + \gamma F' + \delta F'' + \varepsilon F''' + \&c. \ldots\ \mu F^{(n-1)} + \nu F^{(n)} + nu' + \varrho u'' + \sigma u''' + \&c. \ldots = 0$.

If, in particular applications, $\int\frac{(A+Bx^2)\,.\,dx}{\sqrt{(1-e^2x^2)(1-x^2)}}$ represents the arc or area of a curve, the foregoing results, differently expressed, will announce properties subsisting between the arcs and areas of similar curves; for instance, when $A=1$, $B=e^2$, the integral $\int\frac{(1-e^2x^2)\,dx}{\sqrt{(1-x^2)(1-e^2x^2)}}$ expresses the arc of an ellipse, abscissa x, semi-

axes 1 and $\sqrt{(1-e^2)}$; consequently, the arc of one ellipse may be represented by the arcs of other ellipses whose excentricities vary according to a given law; thus,

$$\frac{1-e'}{2}\cdot\int\frac{du'}{U'}, \text{ or } \frac{(1-e')}{2}\cdot\frac{(1+e'')}{2}\int\frac{du''}{U''}=\frac{e^2}{4}\cdot(1+e')\,u'-f+\frac{f'}{1+e'},$$

and $\frac{1-e''}{2}\int\frac{du''}{U''}=\frac{e'^2}{4}(1+e'')\,u''-f'+\frac{f''}{1+e''}$.

Consequently,

$$e'u'-(1+e')f+f'=\frac{1+e''}{2}\cdot\frac{1-e'^2}{1-e''^2}\left\{e''u''-f'(1+e'')+f''\right\},$$

$$\text{or } e'u'-(1+e')f+\frac{3+e''}{2\cdot(1+e'')}\cdot f'-\frac{1-e''}{2(1+e'')^2}f''-\frac{e''(1-e'')}{2\cdot(1+e'')^2}u''=0;$$

which equation, calling u' sin. $2\theta'$, u'' sin. $4\theta''$, agrees with the equation

$$2\cdot(1+c')\,E''=\frac{3+c}{1+c}E'-\frac{1-c}{(1+c)^2}(E+c\,.\,\text{sin.}\,\varphi)+2c\,.\,\text{sin.}\,\varphi,$$

given by LEGENDRE, *Mém. de l'Academie*, 1786, page 657.

If $x=1$, $u'=0$, and $u''=0$; consequently,

$$2(1+e')f(1)-\frac{3+e''}{(1+e'')}\cdot 2f'(1)+\frac{(1-e'')}{(1+e'')^2}\cdot 4f''(1)=0. \qquad (b)$$

Putting $x=\frac{v}{\sqrt{(1+v^2)}}$, we have (see page 246) $df=dv\frac{\sqrt{(1+b^2v^2)}}{(1+v^2)^{\frac{3}{2}}}$

$$=d\left\{v\sqrt{\left(\frac{1+b^2v^2}{1+v^2}\right)}\right\}-\frac{b^2}{4}(1+{}'b)\,dz'+\frac{b^2}{4}(1+{}'b)\frac{dz'}{Z'}-\frac{{}'b^2}{1+{}'b}\cdot\frac{z'^2\,dz'}{Z'},$$

$$\text{or } f=v\sqrt{\left(\frac{1+{}'b^2v^2}{1+v^2}\right)}-\frac{b^2}{4}(1+{}'b)\,z'+\frac{b^2}{4}\cdot(1+{}'b)\frac{1+{}''b}{2}\cdot\frac{dz''}{Z''}+$$

$$\frac{1}{1+{}'b}\,{}'f-\frac{1}{1+{}'b}\cdot z'\sqrt{\left(\frac{1+{}'b^2z'^2}{1+z'^2}\right)}.$$

Similarly, $${}'f=z'\sqrt{\left(\frac{1+{}'b^2z'^2}{1+z'^2}\right)}-\frac{{}'b^2}{4}(1+{}''b)\,z''+\frac{{}'b^2}{4}(1+{}''b).\frac{dz''}{Z''}+$$

$$\frac{1}{1+{}''b}\cdot{}''f-\frac{1}{1+{}''b}\cdot z''\sqrt{\left(\frac{1+{}''b^2z''^2}{1+z''^2}\right)}.$$

Hence, exterminating $\frac{dz''}{Z''}$, there results an equation between f, ${}'f$, ${}''f$, and certain functions of v.

If $v=\frac{1}{\sqrt{b}}$, $z'=\frac{1}{\sqrt{{}'b}}$, $z''=\frac{1}{\sqrt{{}''b}}$, substitute these quantities, and

for f, $'f$, $''f$, put $\frac{f(1)+1-b}{2}$, $\frac{'f(1)+1-'b}{2}$, $\frac{''f(1)+1-''b}{2}$, and there results an equation between $f(1)$, $'f(1)$, $''f(1)$, the same as the one given in the preceding page.

LANDEN, Mem. page 35, and LEGENDRE, *Mém. de l'Acad.* 1786, p. 678, have deduced an equation subsisting between the circumference of a circle and the peripheries of two ellipses, whose excentricities are $\sqrt{\frac{1}{2}}$ and $\frac{\sqrt{(2\sqrt{2})}}{1+\frac{1}{2}\sqrt{2}}$;* but the application of the preceding forms will enable us to express, immediately, the relation between the peripheries of a circle and of two ellipses, the excentricity of one ellipse being assumed of any magnitude; thus, by equation (a), page 242.

$$\int\frac{du'}{U'} = \frac{2}{1+e'}\left\{\frac{e^2 u'}{1+e'} - f + \frac{f'}{1+e'}\right\}; \text{ consequently, since}$$

$$\int\frac{dx}{\sqrt{(1-x^2)(1-e^2x^2)}} = \frac{1+e'}{2}\cdot\int\frac{du'}{U'}, \text{ when } x = 1,$$

$$\int\frac{dx}{\sqrt{(1-x^2)(1-e^2x^2)}} = \frac{2f'(1)}{1-e'} - \frac{1+e'}{1-e'}\cdot f(1);$$

$$\text{but} \int\frac{dx}{\sqrt{(1-x^2)(1-e^2x^2)}}\,(x=1) = \text{P}\,.\,\frac{\pi}{2}, \text{ (page 250)}$$

$$\therefore \text{P}\,.\,\frac{\pi}{2} = \frac{2f'(1)}{1-e'} - \frac{1+e'}{1-e'}f(1);$$

or, $(1-e')\,.\,\text{P}\,.\,\pi - 4f'(1) + 2.(1+e')f(1) = 0$;

or, since $\frac{\pi}{2}$ = quadrant of circle (q) radius $=1$,

$$(1-e')\,\text{P}.\,q - 2f'(1) + (1+e')f(1) = 0.$$

And f, f', may represent arcs of ellipses described on the same semiaxis, major (1), with excentricities equal to e, e' being $= \frac{1-\sqrt{(1-e^2)}}{1+\sqrt{(1-e^2)}}$.

* The semiaxes of the two ellipses compared by LANDEN, are $\sqrt{2}$, 1, and $\frac{1}{\sqrt{2}} + \frac{1}{2}$, $\frac{1}{\sqrt{2}} - \frac{1}{2}$.

It is plain that, by a similar method, we may deduce an equation between $f(1)$, $f'(1)$, and $l.\frac{4}{(^m)b}$.

LEGENDRE puts $c'' = \frac{2\sqrt{c'}}{1+c'}$; therefore f answers to E″ in his equation.

From what has preceded it appears, that the forms

$$f = \frac{e^2}{4}(1+e')u' + \frac{e^2}{4}\cdot\frac{e'(1+e')(1+e'')}{4}u'' + \&c. + \frac{P(1-eQ)}{2^n}\int\frac{dv}{V},$$

$$f = v\sqrt{\left(\frac{1+b^2v^2}{1+v^2}\right)}$$
$$-\left\{\frac{b^2}{4}\cdot(1+{}'b)z' + \frac{b^2\,{}'b\,(1+{}'b)(1+{}''b)}{4\cdot 4}z'' + \&c.\right\}$$
$$+\frac{b\,.\,{}'P\,{}'Q}{2^n}\cdot l.(\zeta+\sqrt{(1+\zeta^2)}),$$

$$f = \frac{e'u'}{1+e'} - \frac{e''(1-e'')\cdot u''}{2\,.\,(1+e')(1+e'')^2} + \frac{3+e''}{2\,.\,(1+e')(1+e'')}\cdot f' - \frac{(1-e'')f''}{2\,.\,(1+e')(1+e'')^2}\,\&c.$$

are parts of the same method of computation, differently expressed. It also appears, how certain analytical artifices of computation, translated into geometrical language, become curious properties of curves.

FAGNANI'S theorem, as it is called, may be deduced from the form for the transformation of f; thus, taking the simplest case,

$$df = \frac{e^2}{4}(1+e')\,.\,du' - \frac{(1-e')}{2}\cdot\frac{du'}{U'} + \frac{1}{1+e'}\cdot df',$$

when u' is at its maximum, (1) $x = \frac{1}{\sqrt{(1+b)}}$

$$\therefore f\left(x = \frac{1}{\sqrt{(1+b)}}\right) = \frac{e^2}{4}\cdot(1+e') - \frac{1-e'}{2}\cdot\int\frac{du'}{U'}(u'=1) + \frac{f'(1)}{(1+e')}$$
$$(u'=1),$$

$$\text{and } f(1)\ (x=1) = \qquad -\frac{1-e'}{2}\cdot 2\int\frac{du'}{U'} + \frac{2f'(1)}{1+e'}.$$

Consequently, $2f - f(1) = \frac{2e^2}{4}(1+e') = (1-b^2)\cdot\frac{1+e'}{2}$

$$= (1-b^2)\frac{1}{1+b} = 1-b;$$

or $2f=f(1)+1-b$, or $f-\left\{f(1)-f\right\}=1-b$,

or $f-\frac{f(1)}{2}=\frac{1-b}{2}$.

This, expressed with reference to an ellipse, announces that the difference between an arc of an ellipse $\left(\text{abscissa}=\frac{1}{\sqrt{(1+b)}}\right)$ and half the quadrant of an ellipse, equals half the difference of the semiaxes.

Similarly, the difference between f and $\frac{f(1)}{4}$ may be asssigned, when $f=\int dx\sqrt{\left(\frac{1-e^2x^2}{1-x^2}\right)}$, $u''=1$, and, consequently, when $u'=\frac{1}{\sqrt{(1+b')}}$ $=\frac{2}{1+e'}\,x\sqrt{\left(\frac{1-x^2}{1-e^2x^2}\right)}$; thus, supposing the value of x to be a, when $u''=1$; since,

$$df=\frac{e'}{1+e'}\cdot du'+\frac{e''}{(1+e')(1+e'')}\cdot du''-\left\{\frac{1-e'}{2}\cdot\frac{1+e''}{2}+\frac{1-e''}{2.(1+e')}\right\}\frac{du''}{U''}$$
$$+\frac{df''}{(1+e')(1+e'')},$$

$$\therefore f(x=a)=\frac{e'}{1+e'}\cdot\frac{1}{\sqrt{(1+b')}}+\frac{e''}{(1+e')(1+e'')}-\left\{\frac{(1-e')(1+e'')}{2\,.\,2}+\right.$$
$$\left.\frac{1-e''}{2\,.\,(1+e')}\right\}\int\frac{du''}{U''}+\frac{f''}{(1+e')(1+e'')},$$

and $f(1)=-\left\{\frac{(1-e')(1+e'')}{2\,.\,2}+\frac{(1-e'')}{2\,.\,(1+e')}\cdot\right\}4\int\frac{du''}{U''}+\frac{4f''(1)}{(1+e')(1+e'')}$,

$$\therefore 4f(x=a)-f(1)=\frac{4e'}{1+e'}\cdot\frac{1}{\sqrt{(1+b')}}+\frac{4e''}{(1+e')(1+e'')}.$$

Now, $1+b'=\frac{(1+\sqrt{b})^2}{1+b}$, $e'=\frac{1-b}{1+b}$, and $e''=\frac{1-b'}{1+b'}$;

consequently,

$$4f-f(1)=2(1-\sqrt{b})\sqrt{(1+b)}+(1-\sqrt{b})^2,$$

or $f-\frac{1}{4}f(1)=\frac{(1-\sqrt{b})\sqrt{(1+b)}}{2}+\left(\frac{1-\sqrt{b}}{2}\right)^2$.

Now, to determine x, we have $u'=\sqrt{\left(\frac{1+e''}{2}\right)}=\frac{2}{1+e'}\cdot x\sqrt{\left(\frac{1-x^2}{1-e^2x^2}\right)}$;

consequently, putting $\sqrt{\left(\frac{1+e''}{2}\right)}=m$,

$x^2=\frac{1+m^2e'-\sqrt{(1-m^2)(1-m^2e'^2)}}{}$, or, putting for m^2, e', their values

$$x^2 = \frac{1}{1+\sqrt{b}}\left\{1 - \frac{\sqrt{b}}{\sqrt{(1+b)}}\right\};$$

which conclusion agrees with LEGENDRE's, obtained by a different process. See *Mem. de l'Academie*, 1786, p. 665,

The foregoing method may be continued at pleasure; thus, if $u'''=1$, then $u''=\sqrt{\left(\frac{1+e'''}{2}\right)}$, and $u'^2 = \frac{1}{1+\sqrt{b'}}\left\{1-\frac{\sqrt{b'}}{\sqrt{(1+b')}}\right\}$; and, putting this value $= m'^2$, x^2 must be determined from the equation $x^2 = \frac{1+m'^2 e' - \sqrt{(1-m'^2)(1-m'^2 e'^2)}}{2}$; and, similarly must the process be conducted, if u^{iv}, or u^{v}, or $u^{\text{vi}}=1$.

These results, applied to an ellipse, cause it to appear, that right lines can be assigned, respectively equal to the difference between an arc and half the quadrant, between an arc and one-fourth of the quadrant, between an arc and one-eighth of the quadrant, &c.

Here may again be remarked, the connexion between the artifices of computation and the properties of curves; for the series expressing $\int dx \sqrt{\left(\frac{1-e^2x^2}{1-x^2}\right)}$ (*cæteris paribus*) converges more quickly, the less x is; consequently, the whole integral is more commodiously calculated by the theorem $f(1) = 2f\left(x=\frac{1}{\sqrt{(1+b)}}\right) - 1 + b$, than if x were put $=1$, in the form of the expansion of $dx\sqrt{\left(\frac{1-e^2x^2}{1-x^2}\right)}$; still more commodiously, by the theorem

$$f(1) = 4f(x=a) - 2(1-\sqrt{b})\sqrt{(1+b)} - (1-\sqrt{b})^2,$$

where $a^2 = \frac{1}{1+\sqrt{b}}\left\{1-\frac{\sqrt{b}}{\sqrt{(1+b)}}\right\}$ is less than $\frac{1}{\sqrt{(1+b)}}$, and so on.

It has been already observed, that the methods of determining f, by f', and f'', or by f', f'', f''', or by f'', f''', &c. as LEGENDRE has done, or by the regular form which the indefinite reduction of f, into $f^{(n-1)}, f^{(n)}$, assumes, are, *au fond*, the same methods; and I purpose now to show that the substitution, which is to be considered as the base and principle of the method, is the same, although dif-

ferently expressed, in the methods of LEGENDRE, of Mr. IVORY, and of Mr. WALLACE, who have learnedly and ingeniously written on this subject.

In order to deduce the relation between three ellipses, LEGENDRE, *Mem. de l'Academie*, 1786, p. 650, assumes

$$(1-b')\ \sin.\ \varphi = \frac{c'^2.\ \sin.\ \varphi'.\ \cos.\ \varphi'}{\sqrt{(1-c'^2\ \sin.\ \varphi')}}.$$

Now, according to this author's notation, $c'^2 = \frac{4c}{(1+c)^2}$, and $1-b' = \frac{2c}{1+c}$; consequently, $\sin.\ \varphi = \frac{2}{1+c} \cdot \frac{\sin.\ \varphi'.\ \cos.\ \varphi'}{\sqrt{(1-c'^2.\ \sin.\ ^2\varphi')}}$, which is precisely the same substitution as $u' = \frac{2}{1+e'}\ x \sqrt{\left(\frac{1-x^2}{1-e^2\ x^2}\right)}$.

In the Edinb. Trans. Vol. IV. p. 183, $\sin.\ (\psi-\varphi)$ is assumed $= c\ .\ \sin.\ \psi$; but $\sin.\ (\psi-\varphi) = \sin.\ \psi\ .\ \cos.\ \varphi - \cos.\ \psi\ .\ \sin.\ \varphi$,

$$\therefore\ \sin.\ ^2\psi = \frac{\sin.\ ^2\varphi}{1+c^2-2c\ .\ \cos.\ \varphi} = \frac{\left(2\ .\ \sin.\ \frac{\varphi}{2}\ .\ \cos.\ \frac{\varphi}{2}\right)^2}{1+c^2-2c\left(2\left(\cos.\ \frac{\varphi}{2}\right)^2-1\right)};$$

consequently, puttting $\frac{4c}{(1+c)^2} = e^2$,

$$\sin.\ \psi = \frac{2\ .\ \sin.\ \frac{\varphi}{2}\ .\ \cos.\ \frac{\varphi}{2}}{(1+c)\sqrt{\left(1-e^2.\left(\cos.\ \frac{\varphi}{2}\right)^2\right)}},$$ the same substitution as

$$u' = \frac{2}{1+e'}\ .\ x \sqrt{\left(\frac{1-x^2}{1-e^2\ x^2}\right)}.$$

Again, in Edinb. Trans. Vol. V. p. 272, $\sin.\ 2\varphi'$ is made $= \frac{\sin.\ 2\varphi}{\sqrt{(1+e'^2+2e'\ .\ \cos.\ \varphi)}} =$, consequently, $\frac{2\ .\ \sin.\ \varphi\ .\ \cos.\ \varphi}{\sqrt{1+e'^2+2e'\ (1-2\ \sin.\ ^2\varphi)}} = \frac{2}{1+e'} \cdot \frac{\sin.\ \varphi\ .\ \cos.\ \varphi}{\sqrt{(1-e^2\ \sin.\ ^2\varphi)}}\ \left(e^2 = \frac{4e'}{(1+e')^2}\right)$, the same substitution as $u' = \frac{2}{1+e'} . x \sqrt{\left(\frac{1-x^2}{1-e^2\ x^2}\right)}$.

It appears, then, that the preceding substitutions, although, by the aid of geometrical language, differently expressed, are all reducible to the algebraical substitution of $u' = \frac{2}{1+e'} . x \sqrt{\left(\frac{1-x^2}{1-e^2\ x^2}\right)}$, in the

form $dx\sqrt{\left(\frac{1-e^2x^2}{1-x^2}\right)}$; which substitution I conceive to be more obvious, more easily suggested, and more analogous to ordinary algebraical substitutions, than the substitution of $\frac{\sin. 2\phi}{\sqrt{(1+e'^2+2e'.\cos\varphi)}}$ for the sin. $2\varphi'$, or, of $\frac{\sin.(\psi-\varphi)}{c}$ for sin. ψ.

Of this substitution of u' for $x\sqrt{\left(\frac{g+hx^2}{e+fx^2}\right)}$, and of the transformation of $dx\sqrt{\left(\frac{e+fx^2}{g+hx^2}\right)}$ into $\text{A}du'+dx'\sqrt{\left(\frac{e'+f'x'^2}{g'+h'x^2}\right)}$, &c. M. LAGRANGE is, I believe, the original author.

When x is called the sine of an arc θ, $dx\sqrt{\left(\frac{1-e^2x^2}{1-x^2}\right)}$ may be expressed by $d\theta\sqrt{(1-e^2\sin.^2\theta)}$, $\frac{dx}{\sqrt{(1-x^2)(1-e^2x^2)}}$, by $\frac{d\theta}{\sqrt{(1-e^2\sin.^2\theta)}}$. LAGRANGE, *Fonct. Analyt.* p. 90, has treated of the integrals of these expressions; as has LEGENDRE, *Mem. de l'Acad.* p. 663, and LACROIX, *Traité du Calcul diff.* Vol. II. page 454.

The results obtained by these authors, may easily be deduced from the substitution of $x\sqrt{\left(\frac{1-x^2}{1-e^2x^2}\right)}=v\sqrt{\left(\frac{1-v^2}{1-e^2v^2}\right)}$. Some of these results may appear curious; but I apprehend, what is chiefly necessary for the solutions of problems in physics and astronomy, into which the expressions $\frac{dx}{\sqrt{(1-x^2)(1-e^2x^2)}}$, $dx\sqrt{\left(\frac{1-e^2x^2}{1-x^2}\right)}$ enter, is a method of approximating to their integrals.

A certain method of approximating to these integrals, has been given in the preceding pages. In different applications, its expression may be varied; thus, f is transformed into an expression involving f', f'', where f', f'', can be more easily computed than f; express this transformation with reference to an ellipse, and it appears that the length of one ellipse may be estimated, from the lengths of two ellipses of different excentricity. Again, $\int\frac{dx}{\sqrt{(1-x^2)(1-e^2x^2)}}$, in order to be computed, is transformed into

$\frac{1+e'}{2}. \int \frac{du'}{\sqrt{(1-u'^2)(1-e'^2 u'^2)}}$, or into $\frac{(1+e')}{2} \cdot \frac{(1+e'')}{2} \cdot \int \frac{du''}{\sqrt{(1-u''^2)(1-e''^2 u''^2)}}$, &c. but $\int \frac{m dx}{\sqrt{(1-x^2)(1-e^2 x^2)}}$ (m a constant quantity) expresses the time of vibration of a pendulum in a circular arc; consequently, the time of vibration of one pendulum may be estimated from the time of vibration of another pendulum, vibrating in a different arc; and, generally, corresponding to relations established between abstract quantities f, f', f'', &c. will be found properties subsisting between those subjects, of which, in particular applications, f, f', f'', &c. become the exponents and expressions.

A certain method for computing the integral of $dx \sqrt{\left(\frac{1-e^2 x^2}{1-x^2}\right)}$ (df) being obtained, in a systematic treatise, the next business of the analyst would be, to show what differential forms depended for their integration on that of df. Such differential forms are many; and, by the introduction of geometrical language, with considerable embarrassment to the computist, varied in their expression. $\frac{dx}{\sqrt{(1-x^2)}(1-e^2 x^2)^{\frac{3}{2}}}$, $\frac{dx}{\sqrt{(1-x^2)} \cdot (1-e^2 x^2)^{\frac{2m+1}{2}}}$, $d\theta \cdot \sqrt{(1+m \cdot \cos. \theta)}$, $\cos. n\theta . d\theta . \sqrt{(1+m \cdot \cos. \theta)}$, $dx \sqrt{\left(\frac{e^2 x^2-1}{x^2-1}\right)}$. ($e, x$ greater than 1) may be reduced to depend for their integration, on $\int \frac{dx}{\sqrt{(1-x^2)(2-e^2 x^2)}}$, and $\int dx \sqrt{\left(\frac{1-e^2 x^2}{1-x^2}\right)}$, ($e, x$ less than 1). Amongst these, $dx \sqrt{\left(\frac{e^2 x^2-1}{x^2-1}\right)}$ merits some attention. In an analytical point of view, there is nothing curious or remarkable in the reduction of such a form to $dx \sqrt{\left(\frac{1-e^2 x^2}{1-x^2}\right)}$, and other quantities that can be integrated; but, with certain conditions, $\int dx \sqrt{\left(\frac{e^2 x^2-1}{x^2-1}\right)}$ represents the arc of an hyperbola; consequently, announcing the analytical result in geometrical language, the hyperbola may be

rectified by means of an ellipse; which property is to be reckoned curious, I conceive, because the ellipse and hyperbola are sections of the same solid cone; for, otherwise, I do not perceive why it is more curious, that an hyperbola should be rectified by means of an ellipse, than that any other curve, whose arc $=$ F, (F an integral dependent on $\int dx\sqrt{\left(\frac{1-e^2x^2}{1-x^2}\right)}$) should be rectified by means of an ellipse.

In order to integrate $dx\sqrt{\left(\frac{e^2x^2-1}{x^2-1}\right)}$ by means of $\int dy\sqrt{\left(\frac{1-e^2y^2}{1-y^2}\right)}$, put $x=\sqrt{\left(\frac{1-\frac{z^2}{e^2}}{1-z^2}\right)}$; then, when $z=0$ $x=1$, and when $z=1$ $x=\infty$, and $dx\sqrt{\left(\frac{e^2x^2-1}{x^2-1}\right)}=\frac{(1-m^2)\,dz}{(1-z^2)^{\frac{3}{2}}\sqrt{(1-m^2z^2)}}$, $\left\{\text{putting } m=\frac{1}{e}\right\}$; consequently, $\int dx\sqrt{\left(\frac{e^2x^2-1}{x^2-1}\right)}$ $(e>1\}$ between the values of $x=0$ and $x=\infty$ $=$ integral of $\frac{(1-m^2)\,dz}{(1-z^2)^{\frac{3}{2}}\sqrt{(1-m^2z^2)}}$ between the values of $z=0$ and $z=1$.

Now, $d\left\{z\sqrt{\left(\frac{1-m^2.z^2}{1-z^2}\right)}\right\}=dz\sqrt{\left(\frac{1-m^2z^2}{1-z^2}\right)}-\frac{(1-m^2)\,dz}{\sqrt{(1-z^2)(1-m^2z^2)}}+\frac{(1-m^2)\,dz}{(1-z^2)^{\frac{3}{2}}(1-m^2z^2)^{\frac{1}{2}}}$.

Hence, $\int\frac{(1-m^2)\,dz}{(1-z^2)^{\frac{3}{2}}\sqrt{(1-m^2z^2)}}=z\sqrt{\left(\frac{1-m^2z^2}{1-z^2}\right)}-\int dz\sqrt{\left(\frac{1-m^2z^2}{1-z^2}\right)}+(1-m^2)\int\frac{dz}{\sqrt{(1-z^2)(1-m^2z^2)}}$;

but, if we put $\int dz\sqrt{\left(\frac{1-m^2z^2}{1-z^2}\right)}=\mathrm{F}$, $\int d\grave{z}\sqrt{\left(\frac{1-\grave{m}^2\grave{z}^2}{1-\grave{z}^2}\right)}=\grave{\mathrm{F}}$,

$m=\frac{1-\sqrt{(1-\grave{m}^2)}}{1+\sqrt{(1-\grave{m}^2)}}$, $z=\frac{2}{1-m}.\grave{z}\sqrt{\left(\frac{1-\grave{z}^2}{1-\grave{m}^2\grave{z}^2}\right)}$.

Then, by equation (a) page 240,

$$\int\frac{dz}{(1-z^2)(1-m^2z^2)}=\frac{2mz}{1-m^2}-\frac{2\grave{\mathrm{F}}}{1-m}+\frac{2\mathrm{F}}{1-m^2};$$

consequently, $\int dx\sqrt{\left(\frac{e^2x^2-1}{(x^2-1)}\right)}=\int\frac{(1-m^2)\,dz}{(1-z^2)^{\frac{3}{2}}\sqrt{(1-m^2z^2)}}$

$$= z\sqrt{\left(\frac{1-m^2 z^2}{1-z^2}\right)} + 2mz - 2(1+m)\,'F + F, \qquad (c)$$

which, in fact, is LANDEN's theorem; for $\int dx \sqrt{\left(\frac{e^2 x^2-1}{x^2-1}\right)}$ represents the arc of an hyperbola, semiaxes 1 and $\sqrt{(e^2-1)}$, and $'F$, F, the arcs of two ellipses.

In an analytical point of view, the latter part of this solution is unnecessary; for the problem is completely resolved, when it is proved that

$$\int \frac{(1-m^2)\,dz}{(1-z^2)^{\frac{3}{2}}\sqrt{(1-m^2 z^2)}} = z\sqrt{\left(\frac{1-m^2 z^2}{1-z^2}\right)} - \int dz \sqrt{\left(\frac{1-m^2 z^2}{1-z^2}\right)} + (1-m^2)\int \frac{dz}{\sqrt{(1-z^2)(1-m^2 z^2)}}.$$

If the differential of $x\sqrt{\left(\frac{1-x^2}{1-e^2 x^2}\right)}$ be taken, it appears that

$$\int \frac{dx}{\sqrt{(1-x^2)}\,(1-e^2 x^2)^{\frac{3}{2}}} = \frac{1}{1-e^2}\int dx \sqrt{\left(\frac{1-e^2 x^2}{1-x^2}\right)} - \frac{e^2}{1-e^2}.x\sqrt{\left(\frac{1-x^2}{1-e^2 x^2}\right)};$$

and hence may be deduced a differential equation of the second order, similar to the one given in page 236. For, since $\frac{df}{dx} = \sqrt{\left(\frac{1-e^2 x^2}{1-x^2}\right)}$, making e only to vary, or taking the partial differentials, $\frac{d^2 f}{dx\,.\,de} = \frac{-ex^2}{\sqrt{(1-x^2)(1-e^2 x^2)}}$, and $\therefore \frac{df}{dx} - \frac{e\,.\,d^2 f}{dx\,.\,de} = \frac{1}{\sqrt{(1-x^2)(1-e^2 x^2)}}$, and $\int \frac{dx}{\sqrt{(1-x^2)(1-e^2 x^2)}} = f - e\,.\,\frac{df}{de}$.

Similarly, $\int \frac{dx}{\sqrt{(1-x^2)}\,(1-e^2 x^2)^{\frac{3}{2}}} = f - e\,.\,\frac{df}{de} - e^2\,.\,\frac{d^2 f}{de^2}$,

or $\frac{f}{1-e^2} - \frac{e^2}{1-e^2}\,x\sqrt{\left(\frac{1-x^2}{1-e^2 x^2}\right)} = f - e\,.\,\frac{df}{de} - e^2\,.\,\frac{d^2 f}{de^2}$,

or $\frac{e^2 f}{1-e^2} + e\,.\,\frac{df}{de} + e^2\,.\,\frac{d^2 f}{de^2} - \frac{e^2}{1-e^2}\,x\sqrt{\left(\frac{1-x^2}{1-e^2 x^2}\right)} = 0$,

when $x = 1$,

$$\frac{e^2}{1-e^2}\,.f(1) + e\frac{df(1)}{de} + \frac{e^2\,.\,d^2 f(1)}{de^2} = 0.$$

I now purpose to show that the integration of forms such as

$\frac{dx}{\sqrt{(1-x^2)}}(1-e^2 x^2)^{\frac{+2m+1}{2}}$, depends on that of $\frac{dx}{\sqrt{(1-x^2)\cdot(1-e^2 x^2)}}$, and of $dx\sqrt{\left(\frac{1-e^2 x^2}{1-x^2}\right)}$.

Let $\frac{dx}{\sqrt{1-x^2}}=d\theta$., $1-e^2 x^2=R^2$ $\therefore$ $x^2=\frac{1-R^2}{e^2}$, $1-2x^2=\frac{e^2-2+2R^2}{e^2}$; consequently,

$$d\left\{x\sqrt{(1-x^2)}\,R^{2m-1}\right\}=$$

$$d\theta\left\{\frac{e^2-2+2R^2}{e^2}\right\}R^{2m-1}-(2m-1)\,d\theta\;R^{2m-3}\left\{\frac{(1-R^2)(e^2-1+R^2)}{e^2}\right\}$$

$$=2m.\left(\frac{e^2-2}{e^2}\right).R^{2m-1}d\theta+\frac{(2m-1)(1-e^2)}{e^2}.R^{2m-3}d\theta+\frac{2m+1}{e^2}.R^{2m+1}d\theta,$$

and $\int R^{2m+1}d\theta=\frac{e^2}{2m+1}.x\sqrt{(1-x^2)}.R^{2m-1}-\frac{2m}{2m+1}(e^2-2).\int R^{2m-1}.d\theta$

$-\frac{(2m-1)}{2m+1}(1-e^2)\int R^{2m-3}.d\theta$; (d)

and, if $(2m+1)$ be negative, either by substitution, or by taking the differential of $\frac{x\sqrt{(1-x^2)}}{R^{2m-1}}$, we have

$$\int\frac{d\theta}{R^{2m+1}}=\frac{2m-2}{2m-1}\cdot\frac{2-e^2}{1-e^2}\cdot\int\frac{d\theta}{R^{2m-1}}-\frac{2m-3}{2m-1}\cdot\frac{1}{1-e^2}\cdot\int\frac{d\theta}{R^{2m-3}}$$

$$-\frac{1}{2m-1}\cdot\frac{e^2}{1-e^2}\cdot\frac{x\sqrt{(1-x^2)}}{R^{2m-1}}.$$

Hence, it is clear that $\int d\theta\,.\,R^{\pm(2m+1)}$ depends on $\int d\theta\,.\,R^{\pm(2m-1)}$, and $\int d\theta\,.\,R^{\pm(2m-3)}$; similarly, $\int d\theta\,.\,R^{\pm(2m-1)}$ depends on $\int d\theta\,.\,R^{\pm(2m-3)}$, $\int d\theta\,.\,R^{\pm(2m-5)}$, &c. consequently, $\int d\theta\,.\,R^{\pm(2m+1)}$ depends on $\int d\theta\,.\,R$ and $\frac{\int d\theta}{R}$.

Examples;

1. Let $2m+1=3$ $\therefore$ $2m=2$,

$$\therefore\int\frac{d\theta}{R^3}=\frac{1}{1-e^2}.\int R\,d\theta-\frac{e^2}{1-e^2}\,x\,\frac{\sqrt{(1-x^2)}}{R}\qquad(e)$$

$$=\frac{1}{1-e^2}f-\frac{e^2}{1-e^2}\,x\sqrt{\left(\frac{1-x^2}{1-e^2 x^2}\right)},$$

when $x=1$, $\int\frac{d\theta}{R^3}=\frac{1}{1-e^2}.f(1)$.

2. Let $2m+1=5 \therefore 2m=4$

$$\therefore \int \frac{d\theta}{R^5} = \frac{2}{3} \cdot \frac{2-e^2}{1-e^2} \cdot \int \frac{d\theta}{R^3} - \frac{1}{3(1-e^2)} \cdot \int \frac{d\theta}{R} - \frac{e^2}{3(1-e^2)} \cdot x \frac{\sqrt{(1-x^2)}}{R^3}$$

$$= \frac{2}{3} \cdot \frac{2-e^2}{(1-e^2)^2} f - \frac{1}{3 \cdot (1-e^2)} \cdot \int \frac{d\theta}{R} - \frac{2}{3} \cdot \frac{2-e^2}{(1-e^2)^2} e^2 \cdot \frac{x\sqrt{(1-x^2)}}{R}$$

$$- \frac{e^2}{3 \cdot (1-e^2)} \cdot \frac{x\sqrt{(1-x^2)}}{R^3};$$

if $x=1$,

$$\int \frac{d\theta}{R^5} = \frac{2}{3} \cdot \frac{2-e^2}{(1-e^2)^2} \cdot f(1) - \frac{1}{3 \cdot (1-e^2)} \cdot \int \frac{d\theta}{R}. \qquad (f)$$

This is as commodious a form as any for computation, but it may easily be changed into others; thus, since $\int \frac{d\theta}{R} = f - e \cdot \frac{df}{de}$,

$\int \frac{d\theta}{R^5} = \frac{3-e^2}{3 \cdot (1-e^2)^2} \cdot f(1) + \frac{e}{3 \cdot (1-e^2)} \cdot \frac{df}{de}$; (integral taken from $x=0$ to $x=1$);

or, since $\int \frac{d\theta}{R} = \int \frac{dx}{\sqrt{(1-x^2)(1-e^2x^2)}} = \frac{1+e'}{2}, \int \frac{du'}{\sqrt{(1-u'^2)(1-e'^2u'^2)}}$

$= \frac{2f'(1)}{1-e'} - f(1) \frac{1+e'}{1-e'}$, by equation ($a$), page 240.

$$\therefore \int \frac{d\theta}{R^5} = \frac{5+3e'^2}{3 \cdot (1-e')^2} \cdot \left(\frac{1+e'}{1-e'}\right)^2 f(1) - \frac{2 \cdot (1+e')^2}{3(1-e')^3} \cdot f'(1),$$

$$\text{or } = \frac{5+3e'^2}{3 \cdot (1-e')^2} \cdot \frac{f(1)}{1-e^2} - \frac{2}{3} \cdot \frac{1}{(1-e^2)(1-e')} \cdot f'(1),$$

the integral being taken from $x=0$ to $x=1$.

The integration of the form $\frac{x^{2n}\,dx}{\sqrt{(1-x^2)}}(1-e^2x^2)^{\frac{\pm 2m+1}{2}}$, depends also on the integration of $R \frac{dx}{\sqrt{(1-x^2)(1-e^2x^2)}}$, and of $dx \sqrt{\left(\frac{1-e^2x^2}{1-x^2}\right)}$; for, substituting as before, and taking the differential of $x^{2n-1}\sqrt{(1-x^2)} \cdot R^{2m+1}$, (X), we have

$$dX = \frac{(2m+2n)\,e^2 - (2m+1)\,x^{2n-2}\,R^{2m+1} \cdot d\theta}{e^2} - (2n+2m+1) \cdot x^{2n} \cdot R^{2m+1} \cdot d\theta$$
$$+ (2m+1)\frac{1-e^2}{e^2} \cdot x^{2n-2} \cdot R^{2m-1} \cdot d\theta.$$

Hence, $\int x^{2n} \cdot R^{2m+1} \cdot d\theta = \frac{(2n+2m)\,e^2 - (2m+1)}{(2n+2m+1)\,e^2} \cdot \int x^{2n-2} \cdot R^{2m+1} \cdot d\theta$

(g)

$$+\frac{2m+1}{2n+2m+1}\cdot\frac{1-e^2}{e^2}\cdot x^{2n-2}\,R^{2m-1}\,d\theta-\frac{X}{2n+2m+1}.$$

And, since a similar form is true for $\int x^{2n-2}\,.\,R^{2m+1}\,d\theta$, and $\int x^{2n-2}\,.\,R^{2m-1}d\theta$, by continuing the process, we must at length arrive at forms such as $\int x^{\circ}.\,R^{2\nu+1}\,d\theta, \int x^{\circ}.\,R^{2\nu-1}\,d\theta$, which have already been shown to be integrable by $\int R d\theta$, and $\int\frac{d\theta}{R}$; or at forms such as $x^{2\sigma}\,R d\theta$, $x^{2\sigma}.\frac{d\theta}{R}$, which are integrable by $\int R d\theta$, $\int\frac{d\theta}{R}$; for, by preceding form,

$$\int x^{2\sigma}\,R d\theta = A\int x^{2\sigma-2}\,R d\theta + B\int x^{2\sigma-2}\frac{d\theta}{R}-x^{2\sigma-1}\sqrt{1-x^2}\,R.$$

Similarly,

$$\int x^{2\sigma-2}\,R d\theta = A'\int x^{2\sigma-4}\,R d\theta + B'\int x^{2\sigma-4}\frac{d\theta}{R}-x^{2\sigma-3}\sqrt{1-x^2}\,R,$$

&c.

and $\dfrac{x^{2\sigma}.\,d\theta}{R}=\dfrac{x^{2\sigma-2}.\,(1-R^2)}{e^2}\cdot\dfrac{d\theta}{R}=\dfrac{x^{2\sigma-2}}{e^2}\cdot\dfrac{d\theta}{R}-\dfrac{x^{2\sigma-2}}{e^2}.R d\theta$

$$\frac{x^{2\sigma-2}.\,d\theta}{e^2.\;R}=\frac{x^{2\sigma-4}.\,d\theta}{e^4.\;R}-\frac{x^{2\sigma-4}}{e^4}.\,R d\theta,$$

&c. &c.

so that, finally, the integrals of $x^{2\sigma}.\,R d\theta.$, $\frac{x^{2\sigma}\,d\theta}{R}$, must be reduced to $\int R d\theta$, and $\int\frac{d\theta}{R}$.

Hence, the integral of a form such as

$$\left\{A+Bx^2+Cx^4+\&c.\right\}\frac{(1-e^2\,x^2)^{\pm 2m+1}}{\sqrt{(1-x^2)}}.\,dx, \text{ depends on } \int R d\theta. \text{ and } \int\frac{d\theta}{R}.$$

If $2m+1$ be negative, or the integral of $\frac{x^{2n}.\,d\theta}{R^{2m+1}}$ be required, then, substituting in the preceding form, or, by a direct process, taking the differential of $\frac{x^{2n-1}\sqrt{(1-x^2)}}{R^{2m-1}}$ (X), there will result,

$$\int\frac{x^{2n}.\,d\theta}{R^{2m+1}}=$$

$$\frac{(e^2-1)(2n-1)+2m-2}{(2m-1)(1-e^2).\,e^2}.\int\frac{x^{2n-2}.\,d\theta}{R^{2m-1}}+\frac{(2n-2m+1)}{(2m-1)(1-e^2).\,e^2}.\int\frac{x^{2n-2}\,d\theta}{R^{2m-3}}-\frac{X}{(2m-1)(1-e^2)};$$

and, consequently, $\int \frac{x^{2n}\, d\theta}{R^{2m+1}}$ finally depends on the integrals of $R d\theta$, and of $\frac{d\theta}{R}$.

The expressions hitherto given, are analytical. By the introduction of geometrical language, there arise forms such as $d\theta\sqrt{(1-e^2 . \sin.^2\theta)}$, $d\theta\sqrt{(1-e^2 \cos.^2\theta)}$, $d\theta\sqrt{\left\{1+\frac{e^2}{1-e^2}, (\cos.\theta)^2\right\}}$, $d\theta\sqrt{(1+m . \cos.\theta)}$, $d\theta\sqrt{(m. \cos.\theta+1)}$, $(\cos.\theta)^n . d\theta\left\{1+m \cos.\theta\right\}^{\frac{\pm 2\nu+1}{2}}$; the integration of which depends on that of $dx\sqrt{\left(\frac{1-e^2 x^2}{1-x^2}\right)}$, and of $\frac{dx}{\sqrt{(1-x^2)(1-e^2 x^2)}}$, as might easily be shewn. I shall, however, omit the proof, and only observe, that this variety of expression, by rendering obscure, or remote, the origin of differential expressions, is rather an inconvenience than a benefit to science.

Before I quit this subject, I wish to shew how, from the preceding integrals and methods, the coefficients in the series $A+B . \cos.\theta + C . \cos. 2\theta +$ &c. the expansion of $(a^2+b^2-2ab . \cos.\theta)^{\frac{2m+1}{2}}$, may be determined and computed.

$\left\{a^2+b^2-2ab . \cos.\theta\right\}^{\frac{2m+1}{2}} = a^{2m+1}\left\{1+\frac{b^2}{a^2}-\frac{2b}{a}\cos.\theta\right\}^{\frac{2m+1}{2}}$

$=(\text{if } \frac{b}{a}=e')\, a^{2m+1}\left\{1+e'^2-2e' . \cos.\theta\right\}^{\frac{2m+1}{2}} = A+B . \cos.\theta + C .$ $\cos. 2\theta+$&c.; consequently, $a^{2m+1} . \int(1+e'^2-2e' . \cos.\theta)^{\frac{2m+1}{2}} d\theta =$ $A\theta + B \sin.\theta + \frac{C . \sin. 2\theta}{2} +$ &c. Let $\theta=\pi$

$\therefore A\pi = a^{2m+1}\int(1+e'^2-2e' . \cos.\theta)^{\frac{2m+1}{2}} d\theta$ (when θ is put $=\pi$).

Now, $1+e'^2-2e' . \cos.\theta = 1+e'^2-2e'\left\{2\left(\cos.\frac{\theta}{2}\right)^2-1\right\}$

$=(1+e')^2\left\{1-\frac{4e'}{(1+e')^2}\left(\cos.\frac{\theta}{2}\right)^2\right\}$, let $\frac{4e'}{(1+e')^2}=e^2$, $\cos.\frac{\theta}{2}=x$

$\therefore 1+e'^2-2e' . \cos.\theta = (1+e')^2\left\{1-e^2 x^2\right\}$,

and $\therefore A\pi = a^{2m+1} . (1+e')^{2m+1} . \int (1-e^2 x^2)^{\frac{2m+1}{2}} . \frac{2dx}{\sqrt{(1-x^2)}}$
$= a^{2m+1} . (1+e')^{2m+1} . \int R^{2m+1} d\theta$, which, by what has preceded, can always be determined by means of $\int R d\theta, \int \frac{d\theta}{R}$. To determine B,
$a^{2m+1} . (1+e')^{2m+1} . R^{2m+1} . \cos. \theta = A . \cos. \theta + B . (\cos. \theta)^2 +$ &c.
$\therefore a^{2m+1} . (1+e')^{2m+1} . \int R^{2m+1} . \cos. \theta . d\theta = A . \sin. \theta + \frac{B \sin. 2\theta}{4} + \frac{B\theta}{2} +$ &c.
making $\theta = \pi$, $\sin. \theta$, $\sin. 2\theta$, &c. $= 0$;
consequently, $(a . (1+e'))^{2m+1} . \int R^{2m+1} . \cos. \theta . d\theta . = B\frac{\pi}{2}$; which integral can always be expressed by finite algebraic forms, and the integrals of $R d\theta, \frac{d\theta}{R}$; for, putting $x = \cos. \frac{\theta}{2}$,
$\cos. \theta = 2x^2 - 1$, we have $R^{2m+1} . \cos. \theta . d\theta = (2x^2 - 1) . R^{2m+1} d\theta$
$= 2x^2 . R^{2m+1} . d\theta - R^{2m+1} d\theta$,
and, generally, to determine the coefficient (N) belonging to $\cos. n\theta$,
$\{a.(1+e')\}^{2m+1} . R^{2m+1} \cos. n\theta = A \cos. n\theta + B \cos. n\theta . \cos. \theta +$ &c.
$+ N . \{\cos. n\theta\}^2 + N' . \cos. n\theta . \cos. (n+1)\theta +$ &c.
multiply each side by $d\theta$, and integrate, making $\theta = \pi$; then, since
$\int \cos. m\theta . \cos. (m \pm p)\theta d\theta = \int \frac{1}{2} \cos. (2m \pm p)\theta d\theta + \int \frac{1}{2} \cos. p\theta d\theta$

$$= \frac{1}{2} \sin. \frac{(2m \pm p)\theta}{2m \pm p} + \frac{1}{2} \frac{\sin. p\theta}{p}$$

$$= \frac{1}{2} . \sin. \frac{(2m \pm p)\pi}{2m \pm p} + \frac{1}{2} \frac{\sin. p\pi}{p} = 0, \text{ and, since}$$

$\int N . (\cos. n\theta)^2 d\theta = \frac{1}{2} \int N d\theta . (\cos. 2n\theta + 1) = \frac{1}{2 . 2n} . N \sin. 2n\theta + \frac{N\theta}{2}$
$= N\frac{\pi}{2} = \{a . (1+e')\}^{2m+1} . \int R^{2m+1} d\theta . \cos. n\theta$, and the integral of $R^{2m+1} \cos. n\theta . d\theta$ can always be determined in terms consisting of finite algebraic quantities, and of the integrals $\int R d\theta, \int \frac{d\theta}{R}$; for,

if x be cosine of $\frac{\theta}{2}$, $\cos. n\theta =$

$$\tfrac{1}{2}\left\{ (2x^2 - 1)^n - n \,.\, (2x^2 - 1)^{n-2} + \frac{n \,.\, n-3}{1 \,.\, 2} \,.\, (2x^2 - 1)^{n-4} - \text{\&c.} \right\}$$

Example. Suppose $2m + 1 = -3$,

$\therefore A = \frac{1}{\pi\, a^3 \,.\, (1+e')^3} \,.\, \int \frac{d\theta}{R^3}$. Now, by form ($e$), page 262, f being integral of $dx \sqrt{\left(\frac{1-e^2 x^2}{1-x^2}\right)}$, $\int \frac{d\theta}{R^3} = \frac{2f(1)}{1-e^2}\ (x=1)$;

consequently, $A = \frac{2}{\pi\,(a^3\,(1+e')^3)} \,.\, \frac{f(1)}{1-e^2} = \frac{1}{a^3.\,(1+e')\,(1-e')^2} \,.\, \frac{2f(1)}{\pi}$

$$= \frac{2f(1)}{(a+b)\,(a-b)^2 \,.\, \pi};$$

and, to compute this quantity, the series (6), page 244, or the series (9), page 249, may be used; that is, if $\frac{a-b}{a+b}$ be $\geq \sqrt{\tfrac{1}{2}}$, it is most commodious to employ series (6), if $< \sqrt{\tfrac{1}{2}}$, it is most commodious to employ series (9).

To determine B,

$\frac{B\pi}{2} = \frac{1}{a^3 \,.\, (1+e')^3} \,.\, \int \frac{\cos.\,\theta \,.\, d\theta}{R^3}$, but $\frac{\cos.\,\theta \,.\, d\theta}{R^3} = \frac{(2x^2-1)\,2dx}{\sqrt{(1-x^2)} \,.\, R^3}$,

x being the cosine of $\frac{\theta}{2}$.

Now, by form (g), page 263,

$$\int \frac{x^2\, dx}{\sqrt{(1-x^2)} \,.\, R^3} = \frac{1}{e^2.\,(1-e^2)} \,.\, f(1) - \frac{1}{e^2} \,.\, \int \frac{dx}{\sqrt{(1-x^2)}\,(1-e^2 x^2)},$$

$$\text{and} \int \frac{dx}{\sqrt{(1-x^2)}.\,R^3} = \frac{f(1)}{1-e^2};$$

hence,

$$\int \frac{\cos.\,\theta\, d\theta}{R^3} = \frac{2\,(2-e^2)}{e^2.\,1-e^2} f(1) - \frac{4}{e^2} \int \frac{dx}{\sqrt{(1-x^2)}\,(1-e^2\, x^2)}.$$

$$= \frac{(1+e'^2)\,(1+e')^2}{e'.\,(1-e')^2} \,.\, f(1) - \frac{(1+e')^2}{e'} \int \frac{dx}{\sqrt{(1-x^2)}\,(1-e^2\, x^2)}.$$

Hence, calling $\int \frac{dx}{\sqrt{(1-x^2)}\,(1-e^2\, x^2)}$, (from $x=0$ to $x=1$) $F(1)$,

we have $B = \frac{a^2+b^2}{ab \,.\, (a-b)^2\,(a+b)} \,.\, \frac{2f(1)}{\pi} - \frac{1}{ab \,.\, (a+b)} \,.\, 2F(1)$,

if $e' = \frac{b}{a}$ be $\angle \sqrt{2}-1$, compute $f(1)$ from series (6), and $F(1)$ from the series $(1+e')(1+e'')(1+e''') \ldots (1+\varepsilon) \cdot \frac{\pi}{2}$ $\left(\frac{P\pi}{2}\right)$, to which it is equal,

if $e' = \frac{b}{a}$ be $\geq \sqrt{2}-1$, compute $f(1)$ from series (9), and $F(1)$ from the series $(1+{}'b)(1+{}''b)(1+{}'''b) \ldots (1+\beta) \cdot \frac{\text{hyp. log.} \frac{4}{{}^{(m)}b}}{2^m}$ $\left({}'P . \frac{\text{h. l.} \frac{4}{{}^{(m)}b}}{2^m}\right)$, to which it is equal.

For the purposes of computation, the foregoing expression for B is, I believe, as simple as any that can be proposed. It is easy, however, by means of the preceding forms, to express it differently; thus, $\int \frac{dx}{\sqrt{(1-x^2)}\,(1-e^2x^2)}$ (from $x=0$ to $x=1$)

$$= -\frac{1+e'}{1-e'} \cdot f(1) + \frac{1}{1-e'} \cdot 2f'(1).$$

Consequently, $\int \frac{\cos.\theta \cdot d\theta}{R^3}$

$$= \frac{(1+e')^2 . (1+e'^2)}{e'(1-e')^2} \cdot f(1) + \frac{(1+e')^3}{e'(1-e')} \cdot f(1) - \frac{(1+e')^2}{e'(1-e')} \cdot 2f'(1)$$

$$= \frac{2 . (1+e')^2}{e' . (1-e')^2} \cdot f(1) - \frac{(1+e')^2}{e' . (1-e')} \cdot 2f'(1)$$

$$\therefore B = \frac{2}{\pi} \cdot \frac{1}{(a+b)^3} \cdot \int \frac{\cos.\theta \cdot d\theta}{R^3} = \frac{2a}{b . (a+b)(a-b)^2} \cdot \frac{2f(1)}{\pi} - \frac{2}{b(a^2-b^2)} \cdot 2f'(1)$$

$$\text{or } = \frac{2aA}{b} - \frac{2}{b . (a^2-b^2)} \cdot 2f'(1).$$

Let $2m+1 = -1$;

then, $A\pi = \frac{1}{a . (1+e')} \int \frac{2dx}{\sqrt{(1-x^2)(1-e^2x^2)}}$ $(x=1) = \frac{1}{a+b} 2F(1)$,

and $\frac{B\pi}{2} \times a(1+e') = \int \frac{(2x^2-1) . 2dx}{\sqrt{(1-x^2)(1-e^2x^2)}}$

$$= \frac{4}{e^2} \int \frac{dx}{\sqrt{1-x^2}\,(1-e^2x^2)} - \frac{4}{e^2} \int dx \sqrt{\left(\frac{1-e^2x^2}{1-x^2}\right)} - 2\int \frac{dx}{\sqrt{(1-x^2)(1-e^2x^2)}}$$

$$= \frac{2(2-e^2)}{e^2} \int \frac{dx}{\sqrt{(1-x^2)(1-e^2x^2)}} - \frac{4}{e^2} \cdot f = \frac{1+e'^2}{e'} \cdot F(1) - \frac{(1+e')^2}{e'} f(1).$$

Consequently, $B = \frac{a^2+b^2}{ab . a+b} \cdot \frac{2F(1)}{\pi} - \frac{a+b}{ab} \cdot \frac{2f(1)}{\pi}$;

since $F(1) = (1+e')(1+e'') \ldots \frac{\pi}{2} = P \,.\, \frac{\pi}{2}$,

$$A = \frac{1}{a+b} \,.\, P \text{ (if } a \text{ be put } = 1) = \frac{P}{1+e'} = (1+e'')(1+e''') \,\&c.$$

Again, $B = \frac{1+e'^2}{e' \,.\, 1+e'} \,.\, \frac{2F(1)}{\pi} - \frac{1+e'}{e'} \,.\, \frac{2f(1)}{\pi}$;

but $f(1) = \frac{\pi}{2} \,.\, (1-eQ)\, P$,

$$\therefore B = \frac{2P}{(1+e')} \,.\, \left(\frac{2Q}{e} - 1\right)$$

$$\text{or, } = \frac{2P}{1+e'}\left\{\frac{e'}{2} + \frac{e' \,.\, e''}{2 \,.\, 2} + \frac{e' \,.\, e'' \,.\, e'''}{2 \,.\, 2 \,.\, 2} + \&c.\right\},$$

$$\text{or } = (1+e'')(1+e''')(1+e^{\text{iv}}) \,\&c. \left\{ e' + \frac{e' \,.\, e''}{2} + \frac{e' \,.\, e'' \,.\, e'''}{2 \,.\, 2} + \&c. \right\}$$

which agrees with the result given by Mr. IVORY, Edinburgh Transactions, Vol. IV. p. 187.

If, instead of the series used for $F(1)$, $f(1)$, we employ the series $\frac{{}^{\backprime}P}{2^m} \,.\, \text{hyp. log.}\, \frac{4}{{}^{(m)}b}$,

$$1 - b \,.\, \left\{ \frac{b}{2} \,.\, \frac{1+{}^{\backprime}b}{2} + \frac{b \,.\, {}^{\backprime}b \,.\, (1+{}^{\backprime}b)}{2 \,.\, 2 \,.\, 2 \,.} \frac{1+{}^{\backprime\backprime}b}{2} + \&c. \right\} + \frac{b \,.\, {}^{\backprime}P\,{}^{\backprime}Q}{2^m} \,.\, \text{hyp. log.}\, \frac{4}{{}^{(m)}b},$$

we shall obtain expressions for A and B, which, in certain values of b, are more commodious for computation than the preceding expressions.

In like manner, if $2m+1 = -5$,

$$A = \frac{8 \,.\, (a^2+b^2)}{3 \cdot (a^2-b^2)^3 \,.\, (a-b)\, \pi} f(1) - \frac{.\, 2 \,.\, F(1)}{3 \cdot (a^2-b^2)^2 \,.\, (a+b)},$$

$$B = \frac{a}{b} \,.\, \frac{a^4 + 14 a^2 b^2 + b^4}{3 \cdot (a^2-b^2)^4 \, (a+b)\, \pi} f(1) - \frac{a\,(a^2+b^2)}{3 \,.\, b\,(a^2-b^2)^2\,(a+b)} F(1).$$

Since $N = \frac{2}{\pi} \,.\, (a \,.\, (1+e'))^{2m+1} \int R^{2m+1} \,.\, \cos.\, n\theta \,.\, d\theta$; by what has preceded, N may always be determined by a direct process, and independently of the preceding terms. For the purposes of computation, however, it is commodious to deduce N from the

two preceding coefficients ''N, 'N; and the method of deduction nearly the oldest, that of Clairaut,* seems to me the best. It is, in substance, nearly as follows.

$$1+e'^2-2e'.\cos.\theta = 1+e'^2\left(1-\frac{2e'}{1+e'^2}.\cos.\theta\right)$$

$=1+e'^2(1-c.\cos.\theta)=(1+e'^2)V^2$, putting $c=\frac{2e'}{1+e'^2}$, $V^2=1-c.\cos.\theta$.

Hence, $''N\,\frac{\pi}{2}=(1+e'^2)^{\frac{2m+1}{2}}\int V^{2m+1}.\cos.(n-2)\,\theta.d\theta,$

$$'N\,\frac{\pi}{2}=(1+e'^2)^{\frac{2m+1}{2}}\int V^{2m+1}.\cos.(n-1)\,\theta.d\theta,$$

$$N\,\frac{\pi}{2}=(1+e'^2)^{\frac{2m+1}{2}}\int V^{2m+1}.\cos.n\theta.d\theta;$$

consequently, it is necessary to determine

$\int V^{2m+1}.\cos.n\theta.d\theta$ (F'') from $\int V^{2m+1}.\cos.(n-1)\,\theta\,d\theta.$ (F'), and $\int V^{2m+1}.\cos.(n-2)\,\theta.d\theta.$ (F).

Now, $\frac{1}{2}.\cos.n\theta+\frac{1}{2}.\cos.(n-2)\,\theta=\cos.(n-1)\,\theta.\cos.\theta,$

$$\therefore\ \tfrac{1}{2}dF''+\tfrac{1}{2}dF=V^{2m+1}d\theta.\cos.(n-1)\,\theta.\cos.\theta$$

$$=V^{2m+1}d\theta.\cos.(n-1)\,\theta\left(\frac{1-V^2}{c}\right)$$

$$=\frac{dF'}{c}-\frac{\cos.(n-1)\,d\theta}{c}:V^{2m+3};$$

but, $d\left\{V^{2m+3}.\sin.(n-1)\theta\right\}=(n-1)\cos.(n-1)\,\theta.V^{2m+3}d\theta+$

$$\frac{(2m+3)\,c}{4}.\cos.(n-2)\theta.V^{2m+1}-\frac{(2m+3)}{4}c.\cos.n\theta\,V^{2m+1}d\theta.$$

Hence, $\frac{F''}{2}+\frac{F}{2}=\frac{F'}{c}-\frac{\sin.(n-1)\,\theta.V^{2m+3}}{(n-1)\,c}-\frac{(2m+3)}{4\,(n-1)}F''+\frac{2m+3}{4(n-1)}F,$

$\therefore$ when $\sin.(n-1)\,\theta=0$,

$$F''=\frac{4(n-1)\,F'+(2m+3-2(n-1))\,cF}{(2n+2m+1)\,c},$$

or,

$$N=\frac{(4n-4)\,'N+(2m+5-2n)\,c''N}{(2m+2n+1)\,c}.$$

Let $n=2$ $\therefore$ $'N=B$, $''N=2A$, $N=C$,

$$\therefore\ C=\frac{4B+(2m+1)\,2cA}{(2m+5)\,c}=\frac{4B}{(2m+5)\,c}+\frac{(2m+1)\,2A}{2m+5}.$$

* *Mém. de l'Academie*, 1754, page 550.

Let $n=3$, then,

$$D=\frac{8C}{(2m+7)c}+\frac{(2m-1)B}{2m+7},$$

$$n=4,\quad E=\frac{12D}{(2m+9)c}+\frac{(2m-3)C}{2m+9}$$

&c.

Since, by the preceding forms, the coefficients A, B, can always be expressed in finite algebraic terms, and in terms involving $\int R d\theta$, $\int \frac{d\theta}{R}$, the problem, that of expanding $(1+e'^2-2e'.\cos.\theta)^{\frac{2m+1}{2}}$, is resolved in its most extensive sense. A and B, however, can be determined most easily, in certain values of the index $\frac{2m+1}{2}$; and mathematicians have therefore given methods for deriving A′, B′, (index $\frac{2m+1}{2}\pm 1$) from A, B, (index $\frac{2m+1}{2}$). A method as eligible as any, depends on a problem similar to the preceding; thus, we may determine A′, B′, from A, B, by deducing the integrals of $V^{2m-1}d\theta$, $V^{2m-1}.\cos.\theta.d\theta$, from those of $V^{2m+1}.d\theta$, $V^{2m+1}.\cos.\theta.d\theta$; or, since

$$A\pi = a^{2m+1}.(1+e')^{2m+1}.\ R^{2m+1}.d\theta,$$

$$\frac{B\pi}{2}=a^{2m+1}.(1+e')^{2m+1}.R^{2m+1}.\cos.\theta.d\theta,$$

$$A'\pi=\frac{a^{2m+1}.(1+e')^{2m+1}}{a^2.(1+e')^2}.R^{2m-1}d\theta,$$

$$B'\frac{\pi}{2}=\frac{a^{2m+1}.(1+e')^{2m+1}}{a^2.(1+e')^2}.R^{2m-1}.\cos.\theta.d\theta;$$

and, since $\cos.\theta=2x^2-1$ $\left(x=\cos.\frac{\theta}{2}\right)$.

By substituting, in form *(g)*, page 263, for $2n$, 1, we have

$$\int x^2 R^{2m+1}d\theta=\frac{(2m+2)e^2-(2m+1)}{(2m+3)e^2}.\int R^{2m+1}d\theta+\frac{(2m+1)(1-e^2)}{(2m+3)e^2}.\int R^{2m-1}d\theta\ (x=1)$$

$$\therefore \int(2x^2-1)R^{2m+1}d\theta$$

$$=\frac{(2m+1)e^2-2(2m+1)}{(2m+3)e^2}\int R^{2m+1}d\theta+\frac{2.2m+1}{2m+3}.\frac{1-e^2}{e^2}\int R^{2m-1}d\theta.$$

Consequently,

$$\frac{(2m+3)}{2}e^2 B=(2m+1)(e^2-2)A+2.(2m+1)(1-e^2)a^2.(1+e')^2 A',$$

$$\text{or } \frac{(2m+3)\,2ab}{(a+b)^2}.B=-\frac{(2m+1)\,2.(a^2+b^2)}{(a+b)^2}A+2.(2m+1)(a-b)^2 A',$$

$$\text{or, } A'=\frac{2m+3}{2m+1}.\frac{ab}{(a^2-b^2)^2}B+\frac{a^2+b^2}{(a^2-b^2)^2}A.$$

Again, since $(2x^2-1)R^{2m-1}d\theta=\frac{2-e^2}{e^2}R^{2m-1}d\theta-\frac{2R^{2m+1}}{e^2}.d\theta$ (since $x^2=\frac{1-R^2}{e^2}$) $=\frac{a^2+b^2}{2ab}.R^{2m-1}d\theta-\frac{(a+b)^2}{2ab}R^{2m+1}d\theta$,

$$B'(a+b)^2=\frac{a^2+b^2}{2ab}.(a+b)^2\,2A'-\frac{(a+b)^2}{2ab}2A;$$

and substituting for A',

$$B'=\frac{2m+3}{2m+1}.\frac{a^2+b^2}{(a^2-b^2)^2}B+\frac{4ab}{(a^2-b^2)^2}A.$$

The method of deducing the coefficients, by a direct process of integration, from $R^{2m+1}.d\theta$, cos. $\theta.R^{2m+1}d\theta$, &c. differs, when examined, scarcely at all from the method of determining A and B (index $2m+1$) from A' and B' (index $(2m+1)-2$); for, in the first method,

$$\int R^{2m+1}d\theta=\alpha\int R^{2m-1}d\theta+\alpha'\int R^{2m-3}d\theta+\&c.+{}'\alpha\int Rd\theta+{}''\alpha\int\frac{d\theta}{R}$$
$(x=1)$;

or, by continued reduction,

$=\beta\int Rd\theta+\gamma\int\frac{d\theta}{R}$; (the Greek characters denoting constant coefficients;)

but, since $\int R^{2m-1}d\theta$, $\int R^{2m-3}d\theta$ &c., $\int Rd\theta$; $\int\frac{d\theta}{R}$, multiplied into certain constant quantities are respectively equal to the coefficients, A', A'', A''', &c. ..... $''A$, $'A$, the indices being

$$2m-1,\ 2m-3,\ 2m-5,\ \ldots\ldots\ 1,\ -1,$$

it is clear, that by determining $\int R^{2m+1}d\theta$, from $\int R^{2m-1}d\theta$, &c. we, in other words, determine A by A', A'' ...... $''A$, $'A$, or, when A', A'', &c. are reduced to depend on $''A$, $'A$, by $''A$, $'A$.

By the method given in the preceding pages, the coefficients are made to depend on the integrals (f, F) of $dx\sqrt{\left(\frac{1-e^2x^2}{1-x^2}\right)}$, $\frac{dx}{\sqrt{(1-x^2)\,1-e^2x^2)}}$. These integrals, it is necessary to compute; and methods have been given for that purpose for all values of e, and consequently for all values of a and b. If the coefficients are to be determined by deriving A, B, from A′, B′, &c. the best method to be followed, is that given by Mr. IVORY, who determines the coefficients, when the index $2m+1=-1$, in fact, by integrating $\frac{dx}{\sqrt{(1-x^2)(1-e^2x^2)}}$, ($d$F), or $\frac{d\theta}{\sqrt{(1-e^2(\sin.\theta)^2}}$, on which A depends, and $dx\sqrt{\left(\frac{1-e^2x^2}{1-x^2}\right)}$ (df), or $d\theta\sqrt{(1-e^2.(\sin.\theta)^2}$, on which B partly depends.*

The author last mentioned, in his valuable Paper inserted in the Edinb. Transactions, first, I believe, applied the method of transforming f, F, into similar integrals f', F′, f'', F″, &c. to the determination of the coefficients A, B, &c.; but the method of transformation belongs to LAGRANGE.† This great mathematician has also solved the problem of the expansion of $(a^2+b^2-2ab.\cos.\theta)^{\frac{2m+1}{2}}$; he determines A and B, when the index $\frac{2m+1}{2}=\frac{1}{2}$, in which case, the series for A and B, with respect to its numerical coefficients, decreases the fastest. But the solution is not general, or, to speak

* B depends on f and F, for

$$\frac{\cos.\theta\,.\,d\theta}{\sqrt{(1-e^2.\left(\cos.\frac{\theta}{2}\right)^2}}=\frac{(2x^2-1)\,2dx}{\sqrt{(1-x^2)\,(1-e^2x^2)}}=\frac{-4}{e^2}.\frac{(1-e^2x^2)\,dx}{\sqrt{(1-x^2)\,(1-e^2x^2)}}$$

$$+2.\frac{(2-e^2)}{e^2}.\frac{dx}{\sqrt{(1-x^2)\,(1-e^2x^2)}}=\frac{-4}{e^2}dx\sqrt{\left(\frac{1-e^2x^2}{1-x^2}\right)}+\frac{2.(2-e^2)}{e^2}\frac{dx}{\sqrt{(1-x^2)(1-e^2x^2)}}.$$

† I assert this on the authority of LACROIX, having never been able to procure the volume of the Turin Memoirs in which LAGRANGE's method is contained.

more precisely, it would be extremely incommodious to compute A and B from the series ascending by the powers of $\frac{b}{a}$, if b were nearly $= a$.

The method of LAGRANGE, given in the Berlin Acts for 1781, p. 252, has been followed by LAPLACE, *Mécanique céleste*, p. 268; and, in that part which relates to the derivation of A, B, from A′, B′, by LACROIX, *Calc. diff.* p. 120; and by Mr. WALLACE, Edinb. Transactions, Vol. V. p. 256. But the great difficulty of the problem does not consist in deriving the coefficients from one another, but in computing the value of the first and second; and, for this end, a series that simply expresses the expansion of $(1 - e^2.(\cos.\theta)^2)^{\frac{2m+1}{2}}$ must be inadequate, at least, it cannot be commodious and general.

CLAIRAUT has given a peculiar method for finding A, *Mém. de l'Acad.* 1754, p. 546. ARBOGAST, *Calcul des Derivations*, p. 359, has given a form for the expansion of $(1 - c.\cos.\theta)^{\frac{2m+1}{2}}$, which agrees with LACROIX's, *Calc. int.* p. 121; but the expansion is inconvenient, for reasons already stated, for the purposes of arithmetical computation.

If we join together certain parts of LEGENDRE's Memoir, we shall obtain a complete solution of the problem of the expansion of $(a^2 + b^2 - 2ab(\cos.\theta)^{\frac{2m+1}{2}}$; for he shows, that E, the integral of $d\theta.\sqrt{(1 - e^2(\cos.\theta)^2)}$ may always be resolved into similar integrals E′, E″, or ′E, ″E, or, by continuing the resolution, into E″, E‴, or ″E, ‴E, &c. and, consequently, he shows how E may, in all values of e, be computed; and moreover, he shows that the integral of $(\cos.\theta)^n.(1 + \alpha.\cos.\theta)^{\frac{2m+1}{2}}d\theta$, may be always reduced to that of $(1 + \alpha.\cos.\theta)^{\frac{2m+1}{2}}d\theta$, and therefore to the integral of

$d\theta\sqrt{(1+\alpha\cos.\theta)}$ and $\frac{d\theta}{\sqrt{(1+\alpha\,.\cos.\theta)}}$. Now, the coefficient affecting $\cos.n\theta = a\int\cos.n\theta.d\theta.(1-c.\cos.\theta)^{\frac{2m+1}{2}}$, ($a$ a constant quantity,) and $\cos.n\theta=\frac{1}{2}\{(2.\cos.\theta)^n - n.(2.\cos.\theta)^{n-2} + \frac{n.n-3}{1.2}(2\cos.\theta)^{n-4} - \&c.\}$

E, E′, &c. LEGENDRE calls ellipses, because the differential of the arc of an ellipse may be represented by an expression as $d\theta\sqrt{(1-e^2(\cos.\theta)^2)}$; but the problem of the expansion of $(a^2+b^2-2ab\cos.\theta)^{\frac{2m+1}{2}}$ requires only the integrals of $\frac{d\theta}{R}$, $d\theta.R$; the determination of which integrals, is totally independent of ellipses, as it is, of all other curves.

That the determination of the coefficients A, B, &c. depended on the integral of $Rd\theta$, which, in a particular application, represents the arc of a conic section, was known to D'ALEMBERT. In the *Récherches sur différens Points importans du Système du Monde*, page 66, he proves that A, B, are respectively equal to $\frac{2M}{c}$, $\frac{2M'}{c}$, c being the semicircumference of a circle, and M, M′, the integrals of $dz(a+b\cos.z)^{\frac{n}{2}}$, and $\cos.z.dz.(a+b\cos.z)^{\frac{n}{2}}$, when $z=\frac{c}{2}$; which integrals depend, he says, on the rectification of the conic sections; he then adds a remark, which requires some comment and explanation. "Tout se reduit donc à trouver par approximation, "la rectification d'un arc donné dans une section conique; et c'est "à quoi on peut parvenir aisément par différentes méthodes. Mais "je ne m'étendrai pas davantage là-dessus, parce que cette manière "de trouver les inconnues A et A′, me paroit plus curieuse et plus "géometrique que commode pour le calcul." p. 67. D'ALEMBERT, therefore, rejects a method which has since been adopted: the reason, I presume to be this; if he had attempted to find the coefficients by the rectification of the conic sections, he must have reduced the integrals of $dz(a+b.\cos.z)^{\frac{n}{2}}$, $\cos.zdz.(a+b\cos.z)^{\frac{n}{2}}$

to a series of terms, as $Z' + Z'' + Z'''$ &c. $+\int dz\,(a+b\,.\cos.z)^{\frac{1}{2}} + \int\frac{dz}{(a+b\cos.z)^{\frac{1}{2}}}$; and, after this reduction, he must have found the integral of $dz\,(a+b\cos.z)^{\frac{1}{2}}$, which, as he then could only do by resolving it into a series, was a problem not more easy, than the finding of the integral of $dz\,(a+b\cos.z)^{\frac{n}{2}}$ from its immediate resolution into a series; consequently, the reduction of $dz\,(a+b\cos.z)^{\frac{n}{2}}$, into $Z' + Z'' +$ &c. would have been useless and unprofitable labour. Had a certain and easy method of computing $(a+b.\cos.z)^{\frac{1}{2}}$ been known to D'ALEMBERT, he would not have asserted the reduction of $\int(a+b\,.\cos.z)^{\frac{n}{2}}$ into $Z' + Z'' +$ &c. $\int(a+b\cos.)^{\frac{1}{2}}\,dz$, to be a method "plus curieuse que commode."

The integral of $\frac{x^4\,dx}{\sqrt{(1-x^2)}}$ furnishes an easy instance for illustration. Suppose it were necessary to compute it, a value of x being given less than 1; resolve it into

$$\sqrt{(1-x^2)}\left\{Ax^3 + Bx^2 + Cx\right\} + \int E\,.\frac{dx}{\sqrt{(1-x^2)}};$$

then, from such expression, may the integral be easily found; since we have tables that exhibit the value of $\int\frac{dx}{\sqrt{(1-x^2)}}$ for all values of x between 0 and 1; but, if the zeal and ability of former computists had not enabled us, in all cases, to assign the value of $\int\frac{dx}{\sqrt{(1-x^2)}}$, it would be, practically, more easy and convenient, for a single instance, to compute an expression as $\int\frac{x^4\,dx}{\sqrt{(1-x^2)}}$ immediately from

$$\int x^4\,dx\left\{1 - \text{D}\,1^{\overline{-\frac{1}{2}}}x^2 + \underset{c}{\text{D}^2}\,1^{\overline{-\frac{1}{2}}}x^4 - \underset{6}{\text{D}^3}\,1^{\overline{-\frac{1}{2}}}x^6 + \text{\&c.}\right\},$$

$$\text{or } \frac{x^5}{5} - \frac{\text{D}\,1^{\overline{-\frac{1}{2}}}.\,x^7}{7} + \frac{\underset{c}{\text{D}^2}\,1^{\overline{-\frac{1}{2}}}\,x^9}{9} - \text{\&c.}$$

than from $\sqrt{(1-x^2)}\left\{Ax^3 + Bx^2 + Cx\right\} + E\int\frac{dx}{\sqrt{(1-x^2)}}$,

since, after this reduction, it would be necessary to compute $E\int\frac{dx}{\sqrt{1-x^2}}$ from $E\left\{x - D\,1^{-\frac{1}{2}}\frac{x^3}{3} + \underset{c}{D}^2\,1^{-\frac{1}{2}}.\frac{x^5}{5} - \text{\&c.}\right\}$

These observations are, however, digressive; the problem, the expansion of $(a^2+b^2-2ab\,.\cos\theta)^{\frac{2m+1}{2}}$, is, I conceive, completely resolved in the preceding pages, whatever be the ratio between the radii of the planets' orbits.*

What I have advanced, on a former occasion, concerning the independence of analysis and geometry, is confirmed by the present reasonings and results. $\int dx\sqrt{\left(\frac{1-e^2x^2}{1-x^2}\right)}$, $\int.dx\sqrt{\left(\frac{e^2x^2-1}{x^2-1}\right)}$, $\int\frac{dx}{\sqrt{(1-x^2)(1-e^2x^2)}}$, have been computed, without the introduction of an ellipse, an hyperbola, an oblique cylinder, or a pendulum

* In the case of the new planets Ceres and Pallas, whose mean distances from the sun are nearly equal, the series (8), and the expression $\frac{(1+{}'b)(1+{}''b)(1+{}'''b)\ldots(1+{}^{(m)}b)}{2^m}\,.\,l.\,\frac{4}{{}^{(m)}b}$ (F), will be very convenient, on account of the rapid convergency of the quantities, $'b$, $''b$, $'''b$, &c.; and, in general, in estimating the disturbing forces of 2 planets, since e' is

$$=\frac{\text{mean distance of nearest planet}}{\text{mean distance of the more remote planet}},\text{ and}$$

$$b=\frac{1-e'}{1+e'},\text{ putting } b=e',\ e'=\sqrt{2}-1=.4142,\text{ \&c.}$$

hence, if e' be greater than .4142, &c. the series of terms $'b$, $''b$, $'''b$, &c. decrease more rapidly than e'', e''', &c. and, consequently, the series (8), page 248, and the series $\frac{(1+{}'b)(1+{}''b)\ldots(1+{}^{(m)}b)}{2^m}\,.\,l.\,\frac{4}{{}^{(m)}b}$, are to be used in determining the perturbations,

when the planets are	Mercury and Venus,	$e'=0.53516076$
	Venus and Earth,	$e'=0.72333230$
	Venus and Mars,	$e'=0.47472320$
	Earth and Mars,	$e'=0.65630030$
	Jupiter and Saturn,	$e'=0.54531725$
	Saturn and Georgium,	$e'=0.49719638$
	Ceres and Pallas,	

vibrating in a circular arc; and, as $\int \frac{dx}{\sqrt{(1-x^4)}}$ might have been computed, without the introduction of the *Lemniscata*.* I have stated the mode by which analysis may derive aid from geometry; the extent of the aid however is, I conceive, very small; remove the circle, ellipse, and parabola, curves whose properties have been the object of so much investigation, and we only create for ourselves unnecessary and circuitous operations, by introducing curves into the discussion of questions purely analytical. For the purposes of classification, however, curves may not be altogether useless.

The correspondence that has been shown, between the artifices of calculation and the properties of geometrical figures, may be thought, perhaps, curious or remarkable; and the reduction of several methods into one is, I presume, practically and scientifically useful. On similar reductions, the perfection of analysis, to a great degree, depends: for, a frequent result of a careful investigation is, the discovery that methods apparently different, because differently expressed, are founded on the same principle and fundamental notion; but, if examination and study thus diminish the seeming bulk of our knowledge, they, at the same time, increase its precision and purity.

* See EULER's Memoir, *Novi Comm.* Tom. VI. p. 37, &c. Likewise, relative to the subject of this Paper, *Novi Comm.* Tom. XII. *Nova Acta,* Tom. VII. 1778.

XI. *Observations on Basalt, and on the Transition from the vitreous to the stony Texture, which occurs in the gradual Refrigeration of melted Basalt; with some geological Remarks. In a Letter from* Gregory Watt, *Esq. to the Right Hon.* Charles Greville, *V. P. R. S.*

Read May 10, 1804.

SIR, Soho, April 10, 1804.

THE important geological consequences that seem deducible from the changes of texture developed by Sir JAMES HALL's very judicious Experiments on the regulated cooling of melted Basalt, induced me to attempt a repetition of them, some time after the publication of his interesting and ingenious Paper.* I believe that formerly I had the honour of showing you some of the results of my imperfect and diminutive experiments, which only served to afford additional proofs of the transition from the vitreous to the stony texture, which takes place in the gradual refrigeration of glass. Circumstances have prevented my resuming these investigations, till it lately occurred to me that something might be learned, by exposing to the action of heat, a much larger mass of basaltic matter than, as far as I am informed, had ever at one time been subjected to experiment.

One of the common reverberatory furnaces used in iron founderies for the fusion of pig iron, was strongly heated by a fire maintained for several hours. About seven hundred weight

* Published in the Transactions of the Royal Society of Edinburgh, Vol. V.

of amorphous basalt, here called Rowley Rag, was broken into small pieces, and deposited gradually on the elevated part of the interior of the furnace, between the fire and the chimney, from whence, as it melted, it flowed into the deeper part, in which, in ordinary operations, the melted iron is collected. It was observed by the persons attending, that it did not require half the quantity of fuel, to fuse the basalt, that would have been necessary to melt an equal weight of pig iron. When the whole was melted, it formed a liquid glass, rather tenacious, from which a large ladle-full was taken, which, on being allowed to cool, retained the characters of perfect glass. The fire was maintained, though with gradual diminution, for more than six hours; after which time, the draught of the chimney was intercepted, the surface of the glass was covered with heated sand, and the furnace was filled with coals, which were consumed very slowly. It was eight days before the mass in the furnace was sufficiently cool to be extracted, and even then it retained considerable internal heat.

The form of the mass, being given by the bottom of the furnace, was considerably irregular, approaching to the shape of a wedge whose lower angles were rounded. It was nearly three feet and a half long, two feet and a half wide, about four inches thick at one end, and above eighteen inches at the other. From this diversity of thickness, and from the unequal action of the heat of the furnace, too great an irregularity had prevailed in the refrigeration of the glass, to permit its attainment of a homogeneous texture. These circumstances might probably have been counteracted by better devised precautions; but the inequality of the product is not to be regretted, since it has fortuitously disclosed some very singular peculiarities, in the

arrangement of bodies passing from a vitreous to a stony state, which might have remained unobserved, if the desired homogeneity of the result had been obtained. I shall now endeavour to describe the various products of this operation; and I shall also submit to your consideration, some remarks which appear to me to arise naturally from the phenomena I have observed; premising that, except where my opinions are supported by the unequivocal demonstration of facts, I offer them with the utmost deference to the decision of more experienced and judicious mineralogists and geologists.

It may be proper to give a concise description of rowley rag itself, before I consider the products which it yields by igneous fusion. This species of basalt is fine-grained, of a confused crystallized texture; its fracture uneven in small pieces, conchoidal in large pieces. Its hardness superior to common glass, but inferior to feldspar. Its tenacity considerable. Its action on the magnetic needle strong, but without signs of polarity. Its specific gravity, according to my trials, 2.868. Its general colour iron gray, approaching to black. It is opaque; and it reflects light from a number of brilliant points, some of which seem to be feldspar, and others hornblende.*

* "The ragstone has been accurately analysed by Dr. WITHERING, who found "that 1000 parts of it contained 475 parts of siliceous earth, 325 argillaceous earth, "and 200 calx of iron; but this iron seems to me to be in a very small degree of "calcination, from the dark blue colour of the stone, from the rusty colour it assumes "on being exposed to a farther state of calcination by air and water, and from the "magnetic property of the mountains, which, as Dr. PLOT observed, turned the needle "6° from its proper direction. This magnetic property has since been observed in "several basaltic mountains, particularly in the Giant's Causeway in Ireland, and "very remarkably in a basaltic columnar mountain called Compass Hill, in the island

1st. This substance is easily fused into glass, whose texture is completely vitreous, with few air-bubbles. Its fracture undulated conchoidal. Its hardness superior to feldspar, but inferior to quartz. It possesses scarcely any action on the magnetic needle. Its colour is black: it is nearly opaque, being translucent only in very thin fragments. Its specific gravity appears to be 2.743.

2d. The tendency towards arrangement, in the particles of the fluid glass, is first developed by the formation of minute globules, which are generally nearly spherical, but sometimes elongated, and which are thickly disseminated through the mass. The colour of these globules is considerably lighter than that of the glass; they are commonly grayish-brown, sometimes inclining to chocolate brown, and, when they have been formed near the interior surface of the cavities in the glass, they project, and resemble a cluster of small seeds. Their diameter rarely exceeds a line, and seldom attains that size, as, in general, they are so near to one another, that their surfaces touch before they can acquire considerable magnitude. In the process of cooling, they adapt their form to their confined situation, fill up every interstice, and finally present a homogeneous body, wholly unlike glass, and equally unlike the parent basalt. When the union of the little globules has been imperfectly effected, the fracture of the mass indicates its structure, by numerous minute

" Cannay, one of the Hebrides, described by George Dempster, Esq. in the " Transactions of the Society of Antiquaries in Scotland, Vol. I." See Mineralogy of the South-west part of Staffordshire, by James Keir, Esq. F. R. S. published in Shaw's History of Staffordshire, Vol. I.

Mr. Kirwan states the specific gravity of rowley rag, which he calls *ferrilite*, at 2.748; and assigns its melting point at 98° of Wedgwood's pyrometer.

conchoidal fractures, which display the form of each globule. But, if the arrangement has extended a little farther, all these subdivisions are entirely lost; the mass becomes perfectly compact, has an even or a flat conchoidal fracture, is nearly of the same hardness as the glass, is commonly of a chocolate colour, graduating into a brownish-black, and the intensity of the colour increases in proportion to the degree to which the arrangement has extended. Its aspect is rather greasy; and it much resembles some varieties of jasper, in the compactness of its texture, and in its opacity. Its magnetic action is extremely feeble. Its specific gravity appears to be 2.938.

3d. If the mass were now rapidly cooled, it is obvious that the result would be the substance I have just described; but, if the temperature adapted to the farther arrangement of its particles be continued, another change is immediately commenced, by the progress of which it acquires a more stony texture, much greater tenacity, and its colour deepens as these changes advance, till it becomes absolutely black. Sometimes this alteration is effected by a gradual transition, the limits of which cannot be assigned, but more generally by the formation of secondary spheroids, in the heart of the compact jaspideous substance. These spheroids differ essentially from those first described; the centres of their formation are more remote from each other, and their magnitude is proportionably greater, sometimes extending to a diameter of two inches, and seeming only to be limited by contact with the peripheries of other spheroids. They are radiated, with distinct fibres; sometimes the fibres resemble those of brown hæmatites, and sometimes they are fasciculated irregularly, so as to be very similar in appearance to the argillaceous iron ores rendered prismatic by torrefaction. They are

generally well defined, and easily separable from the mass they are engaged in; and often the fibres divide at equal distances from the centre, so as to detach portions of the spheroid in concentric coats. The transverse fracture of the fibres is compact and fine grained; the colour black; and the hardness somewhat inferior to that of the basaltic glass. When two of the spheroids come into contact by mutual enlargement, no intermixture of their fibres seems to take place; they appear equally impenetrable, and, as neither can penetrate, both are compressed, and their limits are defined by a plane, at which a separation readily takes place, and each of the sides is invested with a rusty colour. When several spheroids come in contact on the same level, they are formed by mutual pressure into pretty regular prisms, whose division is perfectly defined; and, when a spheroid is surrounded on all sides by others, it is compressed into an irregular polyhedron.

4th. The transition from this fibrous state to a different arrangement, seems to be very rapid; for the centre of most of the spheroids becomes compact, before they attain the diameter of half an inch. As the fibrous structure propagates itself by radiating into the unarranged mass, the compact nucleus which supplies its place gradually extends, till it finally attains the limits of the spheroids; and the same arrangement pervades the matter comprehended between them. The mass has now assumed a compact stony texture, and possesses great tenacity. Its hardness is somewhat inferior to that of the glass from which it was formed. Its action on the magnetic needle is very considerable. Its specific gravity is 2.938. Its colour is black, inclining to steel gray: it is absolutely opaque, and only reflects light from a few minute points. Though the divisions between

the spheroids are rendered imperceptible to the eye, they are not obliterated, and their rusty surfaces are often disclosed by an attempt to fracture the mass.

5th. A continuation of the temperature favourable to arrangement, speedily induces another change. The texture of the mass becomes more granular, its colour rather more gray, and the brilliant points larger and more numerous: nor is it long before these brilliant molecules arrange themselves into regular forms; and, finally, the whole mass becomes pervaded by thin crystalline laminæ, which intersect it in every direction, and form projecting crystals in the cavities. The hardness of the basis seems to continue nearly the same; but the aggregate action of the basis, and of the imbedded crystals, on the magnetic needle, is prodigiously increased. It appears to possess some polarity; and minute fragments are suspended by a magnet. Its specific gravity is somewhat increased, as it is now 2.949. The crystals contained in it, when examined by a microscope, appear to be fasciculi of slender prisms, nearly rectangular, terminated by planes perpendicular to the axis; they are extremely brilliant; their colour is greenish-black; they are harder than glass, and fusible at the blowpipe; they are suspended by the action of a magnet. They are arranged nearly side by side, but not accumulated in thickness, so that they present the appearance of broad thin laminæ; they cross one another at all angles, but always on nearly the same plane; and the lamina thus formed is often three or four lines long, and from a line to a line and a half broad, but extremely thin.*

* It may be observed, that the cavities which existed in the glass are not obliterated during the subsequent processes, though their interior surfaces undergo some change. The minute globules first formed often become prominent, and project into the cavities.

It seems obvious, that an equalized temperature would have rendered the whole similar to the substance last described; and it may be fairly inferred, that by a continuance of heat, the minute crystals would have been augmented in their dimensions, by the accession of molecules still engaged in the basis, or by the union of several crystals, till they acquired sufficient magnitude for their nature to be absolutely determined by the usual modes of investigation. It is probable, however, if such precautions had been taken as might have secured this degree of perfection in the ulterior result, that the mass would only have exhibited an uniform aspect, and that the interesting initial phenomena would not have been discovered.*

There are some considerations which appear to offer a partial explanation of the formation of the globules, and of the radiated spheroids. It is well ascertained that heat is emitted by all bodies, in their change from a gaseous to a fluid state, and also

These minute points are soon obliterated by the large curves of the fibrous spheroids, which give a mamellated form to the interiors of the cavities; and, when the crystals are generated in the mass, they shoot into some of the cavities, and line them with their brilliant laminæ.

* In this and the succeeding paragraphs, the word molecule is used in the sense assigned to it by HAUY and DOLOMIEU, and is understood to represent the peculiar solids, of definite composition and invariable form, the accumulation of which, forms the crystals of mineral substances. Such molecules, preserving their form and their essential characteristics, may be extracted from most crystals by mechanical division, and may be subdivided as far as our senses can recognise them. Though we cannot by mechanical means directly divide them into their elementary particles, we are enabled to effect this by chemical solution, the only power to which their aggregation yields. It will be evident, from the observations that follow, that I am inclined to adopt the ingenious idea of DOLOMIEU, that many apparently homogeneous rocks are compounds of the minute molecules of several species of minerals; and that, where a suitable opportunity is given, these will develope themselves by the formation of their peculiar crystals.

in their change from a fluid to a solid state. It is reasonable to suppose, that heat may also be emitted in those changes of arrangement which affect the internal texture of a body, after it has attained an apparently solid state. That a succession of such changes does actually take place, appears to me demonstrated by the appearances I have described, and by the increase of specific gravity, which seems to keep pace with the internal changes of the substance. It would appear, that these changes are caused by a gradual diminution of temperature, which permits certain laws to induce peculiar arrangements among the particles of the glass. When several of these particles enter into this new bond of association, they must form a minute point, from which heat must issue in every direction. That heat will gradually propagate itself, till the temperature of the glass is equalized; and then the recurrence of the circumstances which induced the first particles to arrange, will cause other particles to arrange also, which the attraction of aggregation will dispose round the point first formed. A second emission of heat in every direction will take place; the temperature will again be equalized; and again another concentric coat of arranged particles will apply itself to the little globule. But, at the time when the central point of this globule was formed, the equality of temperature, in the mass of glass, would probably cause a number of similar points to be generated. The formation of each must proceed in a similar manner to what I have described, till their surfaces touch, and all the glass be converted into the same substance.

These globules are therefore formed of concentric coats, but they are also radiated. Every one must have remarked the connexion that almost uniformly exists, between the radiated

structure and the formation by concentric coats. There are few radiated substances which are not divisible into concentric fragments; and as few concentric arrangements which are not radiated. Of the first, it may be sufficient to mention hæmatites; of the second, calcareous stalactites. The tendency to this union of structure, may perhaps be produced by the radiation of the emitted heat, or moisture, if the solution be aqueous; and the divisions of the coats will naturally take place at those pauses in the accumulation of particles, which the momentary emission of heat necessarily induced.

If this be allowed to explain the formation of the first series of globules which consolidate into the jaspideous substance, it will also explain the formation of the larger and more distinctly radiated spheroids, which have been already stated to be very easily divisible into concentric fragments. They probably were also formed round a central point, by the accumulation of thin coats; and the tendency to radiation, which seems almost inseparable from this structure, was perhaps aided by the arrangement induced by the emission of heat from every part of the surface of the spheroids. This mode of formation has the advantage of explaining their impenetrability. Had they been generated by radii diverging from a centre, their compactness must have diminished as their diameter increased; but, in the structure which I have supposed, each coat is composed of particles solidly arranged in immediate contact with each other, leaving no spaces for penetration. The same progress is rigidly observed in the extension of the compact nucleus, which always occupies the centre of the radiated spheroids, and finally extends to their peripheries. It observes the concentric divisions of the radiated part with the greatest precision; and the line of their

separation is always perfectly defined. But the state of aggregation into which the substance has now entered, is so perfect as to overcome the operation of the causes which formerly induced the fibrous structure, and the mass remains compact. The only change that the substance afterwards undergoes, consists in the gradual accumulation of the crystalline molecules, and their arrangement, by their individual polarity, into regular solids. This depends on very different laws from those which consolidated the fluid glass, and aggregated its particles into a compact uniform stone.*

The appearances that I have endeavoured to describe, seem deserving of consideration in several points of view. Few things can be more at variance with commonly received opinion, than the diversified succession of changes of structure which this glass exhibits in its passage to a crystallized state. The generation of the globules which unite to form the jaspideous substance, is what we might be prepared to expect, by observing the cooling

* The case is considerably different, where crystals possessing regular forms are generated in glass. The molecules of which they are formed, have doubtless been only suspended in the vitreous medium; and their union is determined by crystalline polarity, which appears to me perfectly distinct from the simple aggregation which changes a fluid into a solid, whether it be homogeneous or compound, which affects the internal arrangement of those bodies, but which never can separate their components into distinct masses, or form them into regular solids. Every molecule, at the moment of its formation, must necessarily be endowed with all the properties it afterwards possesses. The suspension of such molecules in a fluid medium, though it may conceal, cannot alter those properties; and the union of such molecules, to form a regular solid, in no respect alters their individual or aggregate qualities. Whether heat be evolved at the moment of this union, is a question not easily solved; as the crystallizations with which we are familiar are from chemical solutions, in which some of the molecules are generated by the separation of a combined substance, at the moment when others are united by crystalline polarity.

of a common iron furnace flag. But it appears not very obvious to common apprehension, that the species of arrangement requisite to form this intermediary substance, could be compatible with any fluidity permitting farther motion of the molecules of the mass; yet, immediately after the completion of this arrangement, they receive a new disposition, and the radiated fibrous structure commences. Sometimes this pervades even the unaltered glass; but I presume this only to happen where the minute globules first formed were scattered so far asunder, that their centres became fibrous, before their peripheries came into contact. This view of the subject is justified by the analogous operation of the formation of crystals, similar to those described, in the heart of the radiated spheroids, while their exteriors still retained the fibrous texture.

If it be considered as extraordinary, that a change should be effected, converting an apparently solid and homogeneous mass into an accumulation of radiated spheroids, and that these radii should lose their fibrous structure, and assume the texture, aspect, and tenacity, of a compact, hard, and homogeneous stone, it is certainly much more extraordinary, that this stone should permit farther arrangement to proceed, and should enable the crystalline molecules which it contains in a state of confused aggregation, to arrange themselves, and to form crystals, which, although minute, are equal in the perfection of their forms, and in the brilliancy of their natural polish, to the most precious products of crystallization. It is also well deserving of observation, by how regular a march the magnetic influence of the substance keeps pace with the perfection of its arrangement, till it becomes so powerful, that fragments of the regenerated stone are suspended by the attraction of a magnet.

It has been most justly remarked by Mr. SMITHSON, that solution, far from being necessary to crystallization, effectually prevents its commencement; for, while solution subsists, crystallization cannot take place. It may remain a question, whether previous solution be essential, as a preparatory means of obtaining, by subsequent evaporation, or cooling, the small parts of bodies disengaged, so that they may unite to form regular crystals. If by solution be only meant, that simple action of heat, or water, which merely counteracts the force of aggregation, and relieves the molecules from their bond of union with each other, it certainly is a requisite; but if by solution be meant, that action of affinities by which not only the force of aggregation is overcome, but the combinations which constitute the molecules are destroyed, it obviously is not only unnecessary, but prejudicial to crystallization; as a new set of molecules must be formed, by a new combination of the elementary particles, before the formation of regular bodies can commence. The suspension of the molecules ready to crystallize, may be correctly said to be merely mechanical. Though the mechanical action of trituration can never be expected to resolve even the most easily divisible body into its molecules, because the fractures will be at least as frequently across the natural joints as in their direction, yet, even by this rude method, some perfect molecules may be disengaged; for we find, that water passing over large surfaces of siliceous sand, finds some molecules of silex in the state proper for aggregation, and even for crystallization. Mechanical suspension in a fluid medium, of such density that the crystalline polarity may be enabled to counteract the power of gravity, is with justice considered by Mr. SMITHSON

the only requisite for the formation of crystals.* The circumstances I have detailed, appear to me an additional confirmation of this remark, and perhaps go still farther, by showing that even the fluidity (in the common sense of the word) of the suspending medium is not an indispensable condition. For it appears impossible to annex the idea of fluidity to the union of the minute globules which form the jaspideous substance, still less to that substance when formed, and still less to those spheroids whose obstinate impenetrability is so strongly defined. And if, by any power of imagination, these can be supposed to be fluid at the time they retain this conformation, how can it be supposed that the compact hard tenacious stone into which they are changed could retain these characters in a fluid state? Yet the subsequent formation of crystals proves, that either all these contradictions must be, or that the particles of bodies apparently solid must be capable of some internal motion, enabling them to arrange themselves according to polarity, while they are solid and fixed, as far as they have reference to the ordinary characters of fluidity.

Instances even more remarkable have very long been known and authenticated, though perhaps they have not been generally regarded with the attention they deserve. Glass vessels are well known to be convertible into REAUMUR's porcelain, by the internal arrangement of their particles, without losing their external form, and consequently at a temperature very much below that requisite for their fusion. The change of glass into

* See a chemical Analysis of some Calamines, by JAMES SMITHSON, Esq. Philosophical Transactions for 1803, page 27. See also DOLOMIEU, *Journal des Mines*. No. 22, page 53.

Reaumur's porcelain, does not arise from an evaporation of the alkali, as has been alleged, but from a regular arrangement of the molecules of the glass. It commences by the formation of fibres perpendicular to the surface of the glass, and penetrating into it. At nearly the same time, small radiated globules are formed in the interior of the glass, and the union of these with the fibres, by their mutual increase, forms the whole into a new substance; and, if the requisite temperature be longer maintained, the fibres disappear, and the whole becomes fine-grained, and almost compact. This substance, from the improved state of its aggregation, is much stronger and more tenacious than before, and is not fusible at a heat sufficient to fuse the glass it was formed from; but, if that aggregation be once destroyed, the glass resulting from its fusion is equally fusible with the original glass; and a repetition of the process will again form Reaumur's porcelain, which may be again fused, and so on repeatedly, for the quantity of alkali evaporated during the operation is extremely small. The hardness and and brittleness of metals rapidly cooled, contrasted with the softness and tenacity resulting from their gradual refrigeration, are all analogous instances; and all the processes in which annealing is employed, and more remarkably the tempering of steel, are proofs of the internal motions and arrangements of the particles of matter, at temperatures very much below the heat requisite for their fluidity.

Whatever doubts may arise respecting the formation of the crystals, there seems no reason to suppose that their gradual increase would cease, till all the molecules belonging to that species were exhausted, if the temperature favourable to their generation was continued. If the mass was entirely composed

of one species of molecules, it would be resolved into an aggregation of crystals of the same substance; and probably, by a still farther continuation of the process of arrangement, into one crystal, which, though it might not possess a regular external form, would be perfect in its internal structure.

But, if the mass contains two distinct species of molecules, different results must take place, which will be modified by the proportional quantities of the components. As it has been demonstrated by BERTHOLLET, that the attraction of masses of matter are relatively as their quantities, it follows, that unless a very potent counteracting cause be exerted, the most abundant ingredient in the mixture will be the first to crystallize. But this crystallization will not comprehend the whole of its molecules; for, after a certain quantity of them are arranged, the proportions of the remaining fluid are altered; that ingredient which was before the least, may now be equal, or even greatest, and it will exercise its attraction. As the first crystallization, by subtracting a large portion of the fluid particles, must have obliged the molecules of the less abundant substance to approach each other very closely, they may be able to collect themselves entirely in their first attempt to crystallize, or they may form alternate crystallizations with the remaining unarranged molecules of the more abundant substance. However various the species of molecules may be, they will be regulated by analogous laws, and only serve to diversify the generated substances.

It by no means follows, that the crystals afterwards found to be most infusible would be first generated. Their formation does not altogether depend on their greater or less fusibility, but on the relative strength of the attraction which unites them to the matter they are immersed in, and of the polarity which

invites them to crystallize. In all crystallization from compound fluids, the order in which the several bodies crystallize must be determined by their relative quantities and attractions. It is perfectly obvious, that no molecules can form a crystal in a heat sufficient for its fusion; but it by no means ensues, that it will be formed as soon as the molecules are cooled to the point where the crystalline polarity overcomes the disintegrating power of heat; for they may remain suspended in a fluid formed by more fusible bodies, provided this fluid be sufficiently abundant to keep them from contact with each other, for the crystalline polarity appears to exert itself only at extremely small distances. In a mass composed of substances in a state of fluidity, with refractory molecules suspended among them, it is pretty clear, from the preceding paragraph, that the most abundant ingredient will be the first to crystallize. But the removal of a portion of the suspending fluid must bring the refractory molecules nearer together, and perhaps so near that the crystalline polarity may overcome the attraction of the fluid for them; they will therefore crystallize next, and will be followed by the remaining ingredients, in the order their attractions dictate.

As the crystals last formed must necessarily be impressed, at the parts in contact, by the peculiar forms of those which have been first generated, it also follows, if the preceding reasoning be just, that the infusible crystals may be found impressed by the more fusible substance, which crystallized first; and the remaining ingredients of the mixture, which were subsequently arranged, may be moulded on the refractory crystals; and thus, in the same specimen may exist, a refractory substance generated by fire, impressed by more fusible bodies, and impressing

them in its turn. From the same consideration it is obvious, that no crystal can be formed at a temperature above the degree of its fusibility; and that, as a necessary consequence, no crystal which is more fusible than the basis in which it is imbedded, can be formed by igneous operation.

The same laws must regulate the arrangement of aqueous solutions, and of molecules suspended in aqueous solutions. All these are dependant on heat; for we are unacquainted with any fluidity, and consequently with any solution, which heat does not produce. Ice and soda have no more action on each other than soda and quartz: raise the temperature of the ice, and it unites with the soda; raise the temperature of the soda, and it unites with the quartz. Both solutions are effected by heat, of the degrees of which we know neither the beginning nor the end, and are therefore utterly unable to estimate what aliquot part of its scale is adequate to the production of these effects. Probably a very minute one.

A curious diversity may prevail in the products of a compound body subjected to fusion, when absolute solution is produced. When merely simple fusion takes place, the aggregation of the parts only is destroyed: the fluidity arises from the facility with which they move on each other; and a regulated diminution of temperature, by facilitating their reunion, can hardly fail to recompose the same species that formerly appeared to exist in the compound. But, if the molecules have been dissolved and decomposed, and their component particles diffused through the fluid, there seems to be very little probability that any reunion should compose the same molecules. It is more likely that new compounds will be formed, from which new molecules, and of course new crystals, will be generated; and

that, consequently, the same rock may become the parent of very diversified offspring. These will however retain some traces of their origin; for, as there can be no fusion of a compound body imagined, in which the mutual action of the components will not decompose some portion, there can be no solution supposed so perfect that every molecule shall be destroyed. In the first case, there will exist the germs of a new composition; and, in the second, there will remain the relics of the old.

If these observations are correct, considerable utility seems derivable from them, in the explanation of some geological problems. It will appear, that they strikingly illustrate the analogy which exists between the aqueous and igneous formations, and show that precisely the same order and kind of arrangement is followed, in the generation of stony masses from water as from fire; for, the change of structure, which I have observed to be the most inexplicable part of the process by which glass passes into stone, is almost exactly imitated in the formation of calcareous stalactites. Successive depositions of calcareous carbonate, form a stalactite which at first is fibrous. A continuance of the process causes the fibrous structure to disappear, and the stalactite becomes irregularly spathose. The irregularities then vanish, and it becomes perfect calcareous spar, divisible into large rhomboids, with the form peculiar to that mineral; and all the gradations may be found in the same specimen. Nor is this change confined to a few solitary specimens; for, a considerable extent of coast near Sunderland, is formed of a limestone composed of radiated spheroids, from half an inch to three inches diameter, imperfectly united. When one of these spheroids attains something more than the usual magnitude, it becomes compact in the heart; and it is not unusual to discover portions of the rock,

in which the radii have entirely disappeared, and the whole mass has become compact. It is probable that the entire formation of oolithi and pisolithi is owing to the same cause; and that they are prevented from ever arriving at great size, by the union of their surfaces, and their subsequent consolidation into compact limestone, into which they are continually found to graduate.

Hitherto, I have selected instances from substances which have an undisputed claim to an aqueous origin. I shall now, on the authority of DOLOMIEU, instance a similar arrangement, in a substance respecting the origin of which theorists are not agreed. A species of petrosilex is found in the Val de Nido, in Corsica, which contains radiated petrosiliceous glands, from half a line to an inch in diameter. These glands only differ from the basis by their radiated structure, and their colour; and appear to indicate very clearly, that the rock was subjected to a species of arrangement which, if it had been completed, would have changed its nature, and probably would have rendered it porphyritic; for DOLOMIEU observes, that the centre of the glands was often occupied by a small crystal of feldspar.* The extraordinary rock called the globular Granite of Corsica, is an analogous instance. It is composed of crystals of hornblende, feldspar, quartz, and mica, in confused aggregation; and in this basis are immersed spheroids, about an inch and a half or two inches in diameter, composed of concentric alternate coats of quartz and hornblende. The centre is principally occupied by hornblende; this is surrounded by a zone of quartz. These spheroids are radiated to the centre. There can be little doubt that this rock is merely the result of interrupted crystallization; and that, if the process of arrangement had continued, this structure

* DOLOMIEU. *Journal de Physique*, 1794, page 260.

would have disappeared, and the whole rock would have resembled the present basis. Hitherto, this very singular rock has only been found in detached fragments.*

The admission that solution is not a requisite of crystallization, appears to me an important concession in favour of the aqueous system, which has laboured under very great embarrassment, from the difficulty of dissolving quartz. If a very perfect mechanical suspension be all that is requisite, we may cease to wonder at the almost daily formation of petrified wood, (in which, though crystallization does not actually take place, a very perfect arrangement is indicated, by the intimate union of the siliceous particles,) or of hydrophanous semi-opals in the decomposed serpentine of Mussinet, near Turin, or of chalcedony containing drops of water, in the decomposed basalt of Vicenza.

* I shall venture to quote another instance, on the authority of Professor PLAYFAIR. "The salt rock in Cheshire, which lies in thick beds, interposed between strata of an "argillaceous or marly stone, and is itself mixed with a considerable portion of the "same earth, exhibits a very great peculiarity in its structure. Though it forms a "mass extremely compact, the salt is found to be arranged in round masses, of five or "six feet in diameter, not truly spherical, but each compressed by those that surround "it, so as to have the shape of an irregular polyhedron; these are formed of concentric "coats, distinguishable from each other by their colour, that is, probably, by the "greater or less quantity of earth which they contain; so that the roof of the mine, as "it exhibits a horizontal section of them, is divided into polygonal figures, each with "a multitude of polygons without it, having altogether no inconsiderable resemblance "to a mosaic pavement. In the triangular spaces without the polygons, the salt is in "coats, parallel to the sides of the polygons." Illustration of the HUTTONIAN Theory, page 37.

I am informed, that the siliceous deposition at Geyser, is at first a porous friable mass, and that the addition of more molecules renders it fibrous; also that, on a farther addition, the fibrous structure disappears, and the whole assumes the compact even texture of chalcedony or flint. If I am not misinformed, a series of specimens illustrating this transition, existed in the cabinet of the late Dr. HUTTON, of Edinburgh.

I have endeavoured to show, that in the crystallizations resulting from igneous fusion, it is not only possible but probable, that the most infusible substances might not be the first to crystallize; and this appears to involve important consequences, for it partly removes one of the greatest difficulties that embarrasses the igneous theory, by explaining the possibility of refractory substances generated by fire being impressed by the forms of more fusible ones. It seems, however, that the same order of arrangement would prevail in substances that were suspended in a fluid medium, as the degrees of attraction would be the same. In either case, the first step by which the arrangement of an apparently homogeneous mass commenced, would probably be the accumulation of particular molecules into little globules. Such seems to have happened in variolites, and other rocks which contain spherical concretions of a different nature from their basis. Still farther advanced is the arrangement of porphyries: the molecules of one species have assumed a regular crystalline form; and sometimes two or even more varieties of crystals are formed, which remain unmixed in the unarranged basis. If the remaining molecules of that basis are susceptible of crystallization, it may be fairly concluded, that an extension of the process of arrangement would convert the porphyry into granite, or at least into one of the compound aggregates of crystals which constitute the numerous tribes of granites, grünsteins, and sienites; and it seems equally probable that this might be accomplished, whether the molecules were indebted to a suitable temperature, or to an aqueous medium, for the requisite facility of movement.

The formation of granite and other rocks, must however be referred to the ultimate perfection of crystallization, by which

all the molecules have been permitted to arrange. Those granites called porphyritic, in which large crystals of feldspar are imbedded in a basis compounded of the ordinary ingredients of granite in small grains, are apparently generated from a menstruum in which the molecules of one species, being greatly predominant in number to the rest, are the first to exercise their polarity, and constitute large crystals, which are afterwards surrounded by smaller ones, resulting from the successive separations of the remaining elementary molecules.

The changes of the substance that led to the foregoing remarks, serve to show that they are not altogether hypothetical; and any proof that may appear deficient, seems to be provided by the phenomena exhibited by lavas, in which may be observed every step of the passage from the vitreous to the stony, from that to the porphyritic, and finally to the granitic state. The lava of Lipari, which passes from glass to lava, by the generation of minute globules, may be cited, on the authority of Spallanzani, as an instance of the commencement of the process of arrangement;* and, were not their origin still disputed, I might also cite the pitchstone lavas of the Euganean hills. It would

* Spallanzani, *Viaggi alle due Sicilie. Tomo Secondo,* page 238. The whole passage, literally translated, stands thus. " This lava has a basis of feldspar, of a fine " and compact grain, a splintery fracture, rough to the touch, and emitting sparks, " like flint, when struck with steel. It has an ash colour, in some places approaching " to a leaden colour. It is thickly filled with an immensity of little bodies, which " would be distinguished with difficulty, from the resemblance of their colour to that " of their basis, were it not for their globular form. But this lava is joined to a great " mass of glass, which forms a whole with it, without any division or separation " between them; and this lava, which in many places retains its own nature, is in " many other places reduced to glass. Some parts of this glass are filled with the " same little bodies, but other parts are pure glass. This is in general very compact, " has a dead black colour, and breaks rather into irregular pieces than into undulated

appear, that the transition from the stony to the porphyrytic state is rapid, for perfectly homogeneous lavas are among the rarest of volcanic products. The porphyritic lavas are most numerous; and it is needless to detail the varieties they present. But, though the process of arrangement has often only advanced thus far, it has in many instances proceeded much farther, and it is by no means unusual to find the entire basis regularly arranged into crystalline bodies; thus, to cite a well known instance, in many of the ancient lavas of Somma, large augites are imbedded in a crystalline mass, formed of minute crystals of leucite, together with another crystalline substance, whose nature is not perfectly determined.

The casual occurrence of volcanic glass is nowise at variance with this account, as it is sufficiently probable, that some glasses may have a much greater tendency to crystalline arrangement than others possess; and it cannot appear extraordinary, that regular crystals should sometimes be generated, even in the glass, as it is a matter of daily occurrence in artificial glasses, and in furnace slags.

If the distinction attempted to be shown between igneous fusion and solution be established, it may offer a means of accounting for the abundance of peculiar bodies in lava, which do not exist in other situations, or at least are of extremely rare occurrence. For, if the igneous action decomposes the molecules of the substances on which it operates, there seems every

" frrgments, as glass properly does. Besides, it has I know not what of unctuosity to " the touch, and to the eye, which is not perceptible in the more perfect volcanic " glasses. It yields sparks with steel, like the lava; but the lava is wholly opaque, and " the glass, at the angles and thin edges, has considerable transparency. It is only " opaque where the globules are, which appear to be particles of lava."

probability that new compounds may result, dissimilar to any substances we are acquainted with. It would appear, that the necessity of imagining an undiscovered stratum abounding in leucites, chrysolites, and augites, may be dispensed with; and, as I have endeavoured to show the probability that the most infusible substances will not be the first to crystallize, the penetration of refractory leucites by fusible augites, will cease to be an argument against both being generated in the lava. I may also observe, that the same causes which vary the crystallized bodies resulting from igneous solution, must operate upon the unarranged basis; and that the same rock may be fused into lavas extremely dissimilar, as their varieties must depend on the degree of solution which the fusion has accomplished.*

If the analogy attempted to be shown between the aqueous and igneous formation appear founded, the transition from glass to stone can no way affect the great question which has so long divided geologists, about the origin of basalt; for, though it is synthetically demonstrated that basalt may be formed by fire, the converse of that proposition stands supported by strong analogical arguments, and its formation by water must be allowed to be at least equally possible. How far the probabilities derived from the examination of basaltic formations may influence the ultimate decision, is an enquiry in which I shall not now engage; though I cannot avoid recalling to my mind, the numerous

* The evidence of the generation of leucites in the lava which contains them, collected by Leopold de Buch, and Breislac, and finally acquiesced in by Dolomieu, appears so satisfactory, that it can hardly be deemed presumptuous to assume the point as determined. Neither de Buch nor Dolomieu have been able to convince themselves that the augites were also formed in the lava; but I confess myself entirely unable to appreciate the cogency of their arguments, which seem annihilated by the admission they have made in favour of leucites.

instances of petrifactions found in basalt, and, as a counterpoise to that observation, the equally numerous instances in which the heat emanating from it appears to have indurated strata, and coaked beds of coal. One remark may be stated here with propriety, as it arises immediately from the experiment which has occasioned these observations. In the ultimate result of that experiment, the arrangement of the molecules was much more perfect than in the original rock. It might be supposed, that a longer continuance of the suitable temperature was afforded it. This, however, could not be, for the mass was only a few feet long, and a few inches thick; the fire was only maintained a day; and the whole was cooled in a week. But the hill of solid basalt, from which the substance operated upon was taken, is several miles long, and several hundred feet high; and, supposing it to have been irrupted in a state of igneous fusion, it must have required months, nay years, for its refrigeration. How then comes it, that the process of crystallization is so little advanced? How comes the confusion of its texture to indicate the very reverse of the tranquillity and perfection of arrangement, which may be fairly assumed as necessarily attending the extremely gradual changes of so immense a mass?

This objection admits of being obviated, upon the supposition that, in the process of melting, the molecules of the basalt were decomposed; and that the new ones generated were more disposed to crystallize than those whose place they supplied. This explanation is in some degree justified, by the total disappearance of the minute feldspars and hornblende of the basalt; instead of which, the regenerated stone contains thin laminæ of crystals, which are probably augites.

I cannot leave this subject without noticing some particulars,

in which the process of arrangement described in the early part of this Letter, appears to yield a probable explanation of some of the peculiarities of basalt. The general disposition of basalt to divide into globular masses, in decomposing, is too remarkable a fact to have escaped the attention of naturalists; though, as far as I am informed, no satisfactory explication of it has been given. The common effects of decomposition are obviously inadequate; for it is common to see a large block of amorphous basalt separate into numerous balls, after a few months or years exposure to the weather; and, rapid as the process of decomposition has been in the intervening portions, these balls resist its farther progress with uncommon obstinacy. May not this be attributed to the formation of the radiated spheroids, whose occurrence in my experiment I have already mentioned? and may not their greater resistance of weather simply arise from their aggregation being more perfect than that of the incoherent molecules which have filled the intervals between them? Though the radiated structure has disappeared to the eye, these portions of the stone retain the superiority of more perfect internal arrangement; and, if my pigmy experiments could yield spheroids of two inches diameter, there can be no difficulty in supposing that the grand operations of nature may produce them of several feet. The separation of the decomposed fragments in concentric coats, seems easily explained; for I have already pointed out the facility with which the radii of the spheroids separated at nearly the same distances from their centres, and the form of the fragments which resulted, resembling fragments of bombs.*

* Even granite has been frequently observed to affect globular decomposition, and division into fragments of concentric coats. This mode of decomposition extends to

If this idea be not considered as entirely divested of plausibility, I may venture to extend the same principle, to account for the wonderful regularity of the prismatic configuration of basaltic columns, and also for their articulations. If we suppose that a mass of fluid basalt has filled a valley to an indefinite depth and extent, the process of arrangement in its particles must be induced by the removal of its heat or moisture, according as its solution is igneous or aqueous. This can only be done by the action of the atmosphere on its upper surface, and by the ground on which it reposes absorbing the heat or moisture from its under surface. From the variations of the atmosphere, its action must be irregular; and, from the perpetual change of the parts in contact with the heated or moist surface, its operations will always be nearly as active as at first, allowance being made for its variations. But the absorption of the ground will be regular, and regularly diminishing in activity, in proportion as the parts near the mass approach nearer to the same temperature, or same moisture, with the mass above; and its absorptions can only be carried on by its transmission of heat or moisture to the solid rocks below. From these considerations it seems evident, that the arrangement of the part of the basalt near the ground, will be begun with more energy than it can be continued, and that the results will be more slow and regular than the arrangement induced by the perpetual though variable action of the atmosphere. After the first stage in the process of arrangement has been performed, and a stratum, if I may so term it, of the jaspideous substance extended over the surface of the ground, there seems no reason to doubt

so many substances, that WERNER has called the formation it seems to indicate "*abgesonderte stücke*," which has been rendered in English *distinct concretions*.

that a number of radiated spheroids would be generated in it, which would probably have all their centres about the same distance from the ground; and, as the arranging power undergoes a gradual diminution of energy, it is not probable that two rows in height of them should be formed at once, as that would indicate a hasty process, which had prepared a greater mass of matter for their almost simultaneous formation. From these considerations, there seems no improbability in supposing, that in the arrangement of a mass of fluid basalt, a single layer of radiated spheroids would be formed, reposing on the ground which supported the mass.

I have already stated, that when the radii of two spheroids came into contact, no penetration ensued, but the two bodies became mutually compressed, and separated by a plane, well defined, and invested with a rusty colour. I also stated, that when several spheroids encountered, they formed one another into prisms with well defined angles. In a stratum composed of an indefinite number in superficial extent, but only one in height, of impenetrable spheroids, with nearly equidistant centres, if their peripheries should come in contact on the same plane, it seems obvious that their mutual action would form them into hexagons; and, if these were resisted below, and there was no opposing cause above them, it seems equally clear, that they would extend their dimensions upwards, and thus form hexagonal prisms, whose length might be indefinitely greater than their diameters. The farther the extremities of the radii were removed from the centre, the nearer would be their approach to parallelism; and the structure would be finally propagated by nearly parallel fibres, still keeping within the limits of the hexagonal prism with which their incipient formation commenced;

and the prisms might thus shoot to an indefinite length into the undisturbed central mass of the fluid, till their structure was deranged by the superior influence of a counteracting cause, which would be provided by the action of the atmosphere on the upper surface of the basalt. If this arrangement existed, the same cause that determined the concentric fractures of the fibres of the spheroids, would produce convex articulations in the lower joints of the prisms; and, in proportion as the centre from which they were generated became more remote, the articulations would approximate to planes. If the generating centres were not equidistant, the forms of the pillars would be irregular; and the irregularity would be in proportion to the diversity of distance between the centres. If the difference was great, the number of sides would be altered, and they might be found pentagonal, tetrahedral, and trihedral. As the compression of the fibres would be greatest in the level of the generating centres, the lower part of the prisms would be most compact.

All these conditions seem to be fulfilled, in the actual conformation of basaltic columns; for, in every instance I am acquainted with, they appear to have been formed in the tranquil bosom of the mass, as they have been originally masked by amorphous trap, and their prismatic structure is only displayed by the removal of this covering. This has been variously effected, sometimes by the apparent disrupture of rocks, sometimes by the exterior portions of the mass being thrown down by the failure of the ground on which it stood, sometimes by the violence of the waves, and not unfrequently by the working of quarries. In most instances, these operations have only removed the covering from one side of the colonnade; and it remains crowned, and generally surrounded, by an immense amorphous mass.

Where there are two ranges of columns, with an intervening amorphous stratum, it is probable that the upper is the result of a second inundation of fluid basalt. It is well known that basaltic columns are most solid at the bottom; and their convex articulations have been repeatedly observed. Since these considerations occurred to me, I have had no opportunity of examining, whether the divisions approach nearer to the plane surfaces as they recede from the centre from which the prism was generated, nor whether below that centre the convex surface of the articulations is inverted; but I think it by no means improbable, that subsequent observations may establish this to be the case, and thus confer on this hypothesis nearly all the demonstration of which it is susceptible. I may however add, that the phenomena of prismatic division in basaltic veins, perfectly coincide with what might be inferred from the data upon which my reasoning has proceeded. In veins, it is obvious that the refrigerating or absorbing cause must operate with nearly equal force on each side of the vein; and it follows, that two sets of prisms would be generated, which would be horizontal instead of perpendicular, and that, unless a mass of amorphous basalt was interposed between them, they must form a division in the middle of the vein, as, from the mutual impenetrability of their fibres, they could not incorporate. The coincidence of the existing phenomena with these conclusions, is sufficiently remarkable; for, in numerous observations I have made on the basaltic veins which affect the prismatic configuration, I found the prisms were always horizontal, and often, that there were two ranges of them. One of their ends applied to the wall of the veins, the other frequently united to an amorphous mass which separated them; and, when no such

intermedium occurred, there was invariably a division in the middle of the vein. Not unfrequently, the veins contain three sets of prisms; a range of small ones on each side, and of much larger ones in the middle. In this case, the little prisms are always separated from the large ones, and the divisions of the large ones are very irregular.*

After the statement of my opinion, that perfect similarity of structure may exist in the products of aqueous and igneous formation, it will hardly be necessary to conclude these observations with remarking, that I should not consider the establishment of these peculiar modes of arrangement as the slightest demonstration of the igneous origin of basalt. It appears to me, that the truth of my deductions is entirely independent of either theory, and that, if ever the period should arrive when the origin of basalt shall be determined by irrefragable demonstration, the inferences I have drawn may be accommodated with equal facility to either mode of agency.†

* The observations alluded to were made during the course of last summer, (1803,) on the very numerous basalt veins, or, as they are there called, Whin Dykes, which traverse the red sandstone and red sandstone breccia, which forms the greatest part of the coast of the Firth of Clyde, between Greenock and the Largs.

† Mr. KEIR, in his Paper on the Crystallizations formed in Glass, suggests the probability of basaltic pillars being formed by the crystallization of vitreous lavas. See Philosophical Transactions for 1776, Vol. LXVI. page 530.

DOLOMIEU was of opinion, that the prismatic form was peculiar to lavas which had flowed into the sea; and he attributed it to the shrinking of the mass: his description of the appearances exhibited by what he calls the prismatic lavas at the foot of Etna, merits quotation.

"In the lavas of Etna, the form and dimensions of the columns vary as much as "the manner in which they are grouped; hexaedral and pentàedral prisms are most "abundant; then the tetraedral, the triedral, heptaedral, and octaedral. The least I "have seen are only four inches diameter; others are more than three feet; they are

The immerse magnitude of some basaltic columns, the extreme regularity of their prismatic configuration, and the peculiar structure of their articulations, have directed the attention of naturalists to them, much more than to any of the other rocks which affect the columnar form. Yet many of these are sufficiently remarkable to deserve more particular notice than has generally been paid to them; and they afford most illustrative proof, that this configuration is not confined to either the aqueous or igneous formation; for some lavas, universally allowed to

" commonly of a single shoot, which is sometimes 60 feet high; others are divided by " articulations, which are from one to six feet asunder.

" I have more than once observed a large column divide into several smaller in its " upper part. The columns are generally larger near the top than the bottom of the " stream of lava, because they subdivide; and they are always least in that part of the " stream of lava which first entered the water, the refrigeration being more prompt, " and its effects more marked. Sometimes the columns are placed perpendicularly " side by side, and form vertical walls, which are sometimes more than 100 feet high, " and a league long; sometimes they are heaped obliquely, horizontally, and in all " positions. Some, without being divided in their length, are larger at one end than " the other; and then they are arranged like wood piled up, with all the small ends at " one side; sometimes they are formed into pyramidal bundles, by parting from a " common centre; and, finally, there are some which, by their reunion, form large " balls. These radii of lava, which are rather pyramidal than prismatic, resemble " those of the globular pyrites, striated from the centre to the circumference, which " are found in the chalk of Champagne.

" On the shore of la Trezza, near the Mole, there is a very curious group of little " articulated prisms, which issue from a common centre, and form fasciculi singularly " twisted. The articulations are marked, but the species of vertebræ do not separate. " In the heart of the mountain on which stands the Castello di Jaci, there are large " balls, from two to four feet in diameter, resembling in form the large pyrites in the " chalk of Champagne. These balls of lava are formed of pyramidal columns, united " by their points in a common centre." *Catalogue des Laves de l'Etna*, page 453.

The division of the upper part of basaltic columns into several smaller ones, has also been observed in the basaltic columns of Fairhead, by Dr. Richardson. See Nicholson's Journal, 4to. Vol. V. page 321.

be such, are prismatic.* Columns of porphyry are not rare; and, among other places, are found near Dresden, several feet in length, and not more than two inches in diameter. Columns of petrosilex compose a large portion of a mountain near Conistone lake. Very perfect quadrangular prisms of argillaceous schistus are found near Llanwrst. Rubble slate assumes the columnar form at Barmouth. The limestone near Cyfartha, in Glamorganshire, is divided into very regular acute rhomboidal prisms: even the sandstone of the same district is not unfrequently columnar; and one of the beds of gypsum at Montmartre is distinctly divided into pretty regular columns. Sandstone, clay, argillaceous iron ore, and many other substances, become prismatic by torrefaction; and the prisms of starch formed in drying have often been considered as illustrative of basaltic formations.

I am very far from conceiving, that all these configurations are influenced by such systematic arrangements as have determined the form of some basaltic columns. I consider most of them as solely attributable to contraction; which is only a farther

* Almost all the prisms at the foot of Etna, described by DOLOMIEU, are of dubious origin; most of them are probably basalt. The columns of the Vicentine are of the same substance, and so are the prismatic lavas of Auvergne, and of the Vivarais. The bed of lava at la Scala, near Portici, is divided by vertical fissures, which give it the aspect of irregular columns. At Aquapendente, in a quarry of undoubted lava, near the road, are some much more perfect prisms; but the most beautiful I have seen, are the small ones from Ponza. The columns at Bolsena, are said to be basalt. Those of the Euganean hills are very irregular in form; in their texture they are certainly wholly unlike granite, which Mr. STRANGE thought they resembled. I believe them to be lava.

The mention of some columnar formations that follows, is by no means intended as an enumeration of them. I have confined myself to those which I have either inspected in their natural situation, or of which I have seen numerous specimens.

extension of the aggregative force, and must be regulated by the texture, the form, and the position of the mass. Where the texture of the mass is homogeneous, and its contractions uniform, its dimensions may be diminished, without its continuity being destroyed, provided its aggregation be so strong as to overcome the *vis inertiæ* of the mass, and its adhesion to other substances. But, when the resistance is sufficient to overcome the aggregation, the mass will be rent by fissures perpendicular to the direction in which the greatest resistance to its contraction takes place, or, in other words, by fissures perpendicular to its greatest surface; for it is from the extremities of the greatest surface, that the largest quantity of matter must traverse the greatest space, in order that the contraction may be performed without breach of continuity; therefore, if it be an extensive tabular mass, it will be divided into prisms, by fissures perpendicular to its surfaces The power of aggregation would determine these prisms to be hexagonal, as that form contains the greatest quantity of matter in the least surface, of any prisms that can be united without interposing prisms of other forms. But this would require the texture, the contraction, the thickness of the mass, and its adhesion to surrounding substances, to be every where precisely the same; and, as these conditions can never be fulfilled in an extensive formation, all the irregularities that are found must necessarily ensue. The same rule that determines the fissures of a tabular mass to be perpendicular to its surfaces, must determine the rents in a spheroid to be directed from its periphery to its centre.

Though these considerations may be sufficient to explain the tendency to division into prisms, which is so generally extended, and which has produced many of those abortions that have been

dignified with the name of columns, because they have occurred in lavas and in rocks of trap formation, they are utterly inadequate to illustrating the formation of the more perfect basaltic prisms: they offer no means of accounting for the extreme regularity of the sides and the precision of the angles, for the articulations, for the close contact in which the perfect columns are placed to one another, nor for their mutual adhesion, which is so strong, that it often requires considerable violence to separate them. These facts are in absolute contradiction to all idea of retreat or contraction, and seem to me to coincide perfectly with the explanation of their origin which I have already presumed to lay before you.

I have the honour to be, &c.

GREGORY WATT.

XII. *An Analysis of the magnetical Pyrites; with Remarks on some of the other Sulphurets of Iron. By* Charles Hatchett, *Esq. F. R. S.*

Read May 17, 1804.

§ I.

Of the various metallic sulphurets which constitute one of the grand divisions of ores, none appear to be so universally dispersed throughout the globe, as the sulphuret of iron, commonly called Martial Pyrites; for the species and varieties of this are found at all depths, and in all climates and soils, whether ancient, or of alluvial and recent formation. It is remarkable also, that, under certain circumstances, this sulphuret is daily produced in the humid way; an instance of which, a few years back, I had the honour, in conjunction with Mr. Wiseman, to lay before this Society;* and although, in regard to pecuniary value, the pyrites of iron may be considered as comparatively insignificant, yet there is every reason to believe, that in the operations of nature, it is a substance of very considerable importance.

§ II.

The species and varieties of martial pyrites, are in general so well known, and have been so frequently and accurately described, as to figure, lustre, colour, and other external characters,

* Phil. Trans. for 1798, p. 567.

that it would be totally superfluous here to give any detailed account of them. One of the species, however, merits peculiar notice, as possessing the remarkable property of strong magnetic polarity; and, although it has been described by modern mineralogists,* it does not appear to have been as yet subjected to any regular chemical examination; so that, whether it be a sulphuret of iron inherently endowed with the magnetical property, or a sulphuret in which particles of the ordinary magnetical iron ore are simply but minutely interspersed, has to this time remained undecided.

This species is known by the name of Magnetical Pyrites, and is called by the Germans *Magnet-Kies*, or *Ferrum mineralisatum magnetico-pyritaceum.*

It is most frequently of the colour of bronze, passing to a pale cupreous-red.

The lustre is metallic.

The fracture is unequal, and commonly coarse-grained, but sometimes imperfectly conchoidal.

The fragments are amorphous.

The trace is yellowish-gray, with some metallic lustre.

It is not very hard; but, when struck with steel, sparks are produced, although with some difficulty.

It is brittle, and is easily broken.

This pyrites has been hitherto found only in some parts of Norway, Silesia, Bavaria, and especially at Geier, Meffersdorf, and Breitenbrunn in Saxony; but, having received some specimens from the Right Hon. CHARLES GREVILLE, F. R. S. I

* KIRWAN, Vol. II. p. 79. WIDENMANN, p. 792. EMMERLING, 2d edit. Tom. II. p. 286. KARSTEN, p. 48. BROCHANT, Tome II. p. 232.

was struck with their resemblance to the pyrites of Breitenbrunn, which happened at that time to be in my possession; and, upon trial, I found that they were magnetical, and agreed with the latter in every particular. Their magnetic power was such as strongly to affect a well-poized needle, of about three inches in length; a piece of the pyrites, nearly two inches square, acted upon the needle at the distance of four inches.

The powder (which is blackish-gray, with but little metallic lustre) is immediately taken up by a common magnet; but the pyrites does not act thus on the powder, nor on iron filings, unless it has been placed for some time between magnetical bars; then indeed it acts powerfully, turns the needle completely round, attracts and takes up iron filings, and seems permanently to retain this addition to its original power.

In the specimens which I obtained, the north pole was generally the strongest.

This pyrites was found in Wales, about the year 1798, by the Hon. Robert Greville, F. R. S. who sent the specimens above described to his brother, the Right Hon. C. Greville, with the following account.

"It is found in great abundance in Caernarvonshire, near "the base of the mountain called Moel Elion, or probably with "more accuracy Moel Ælia, and opposite to the mountain "called Mynydd Mawr. These mountains form the entrance "into a little close valley, which leads to Cywellin lake, near "Snowdon, a little beyond the hamlet of Bettws.

"The vein appears to be some yards in depth and breadth, "and seems to run from north to south, as it is found on "Mynydd Mawr, which is across the narrow valley, and "opposite to Moel Ælia."

Mr. R. GREVILLE, in another part of his letter, states that copper ore has been worked in several of the adjacent places, and that, many years ago, Capt. WILLIAMS, of Glan yr Avon, employed some miners at the place where this pyrites is found, but the undertaking proved unproductive. Yellow copper ore is certainly in the vicinity; for some portions of it were adhering to the specimens which have been mentioned; and I shall here observe, that the stone which accompanies the magnetical pyrites, is a variety of the lapis ollaris or pot-stone, of a pale grayish-green, containing smooth cubic crystals of common pyrites.

§ III.

From the appearance of those parts of the magnetical pyrites which have been exposed to the weather, it seems to be liable to oxidizement, but not to vitriolization.

The specific gravity, at temperature 65° of FAHRENHEIT, is 4518.

When exposed to the blowpipe, it emits a sulphureous odour, and melts into a globule nearly black, which is attracted by the magnet.

500 grains, in coarse powder, were exposed, in a small earthen retort, to a red heat, during three hours. By this operation, the weight of the powder was very little diminished; neither was there any appearance of sulphur in the receiver, which however smelt strongly of sulphureous acid.

500 grains of the same were put into a flat porcelain crucible, which was kept in a red heat, under a muffle, during four hours. The powder then appeared of a dark gray, with a tinge of deep red, and weighed 432.50 grains. The loss was therefore 67.50

= 13.50 *per cent.* but, upon examining the residuum, I found that only part of the sulphur had been thus separated.

The magnetical pyrites, when digested in dilute sulphuric acid, is partially dissolved, with little effervescence, although there is a very perceptible odour of sulphuretted hydrogen.

The solution is of a very pale green colour.

Pure ammonia produced a dark green precipitate, tending to black; and prussiate of potash formed a very pale blue precipitate, or rather a white precipitate mingled with a small portion of blue. The whole of the latter, however, by exposure to the air, gradually assumed the usual intensity of Prussian blue; and the blackish green precipitate, formed by ammonia, became gradually ochraceous. These effects therefore fully prove, that the iron in the solution was, for the greater part, at the minimum of oxidizement, so as to form the green sulphate, and white prussiate, of iron;* and, consequently, that the iron of the magnetical pyrites is either quite, or very nearly, in the state of perfect metal.

This pyrites, when treated with nitric acid, of the specific gravity of 1.38, diluted with an equal quantity of water, is at first but little affected; but, when heat is applied, it is dissolved, with much effervescence, and discharge of nitrous gas; the effervescence, however, is by no means so violent as when the common pyrites are treated in a similar manner. It is also worthy of notice, that if the digestion be not of too long duration, a considerable quantity of sulphur, *in substance*, is separated; whilst, on the contrary, scarcely any can be obtained from the common pyrites, when treated in a similar manner; although I

* *Récherches sur le Bleu de Prusse,* par M. Proust. *Annales de Chimie,* Tome XXIII. p. 85.

shall soon have occasion to prove, that the real quantity of sulphur is much more considerable in the latter than in the former.

As soon as muriatic acid is poured on the powder of the magnetical pyrites, a slight effervescence is produced, which becomes violently increased by the application of heat; a quantity of gas is discharged, which, by its odour, by its inflammability, by the colour of the flame, by the deposition of sulphur when burned, and by other properties, was proved to be sulphuretted hydrogen.

During the digestion, sulphur was deposited, which so enveloped a small part of the pyrites, as to protect it from the farther action of the acid.

The solution was of a pale yellowish-green colour. With prussiate of potash it afforded a pale blue precipitate, or rather a white precipitate mixed with blue; and with ammonia it formed a dark blackish-green precipitate, which gradually became ochraceous; so that these effects corroborated the conclusions which were founded on the properties of the sulphuric solution, namely, that the iron contained in the pyrites, is almost, if not quite, in the metallic state.

Other experiments were made; but, as they merely confirm the above observations, I shall proceed to give an account of the analysis.

§ IV.

ANALYSIS OF THE MAGNETICAL PYRITES.

A. One hundred grains, reduced to a fine powder, were digested with two ounces of muriatic acid, in a glass matrass placed in a sand bath. The effects already described took

place; and a pale yellowish-green solution was formed. The residuum was then again digested with two parts of muriatic acid mixed with one of nitric acid; and a quantity of pure sulphur was obtained, which, being dried, weighed 14 grains.

B. The acid in which the residuum had been digested, was added to the first muriatic solution; some nitric acid was also poured in, to promote the oxidizement of the iron, and thereby to facilitate the precipitation of it by ammonia, which was added after the liquor had been boiled for a considerable time. The precipitate thus obtained was boiled with lixivium of potash; it was then edulcorated, dried, made red-hot with wax in a covered porcelain crucible, was completely taken up by a magnet, and, being weighed, amounted to 80 grains.

C. The lixivium of potash was examined by muriate of ammonia, but no alumina was obtained.

D. To the filtrated liquor from which the iron had been precipitated by ammonia, muriate of barytes was added, until it ceased to produce any precipitate; this was then digested with some very dilute muriatic acid, was collected, washed, and, after exposure to a low red heat for a few minutes in a crucible of platina, weighed 155 grains. If therefore the quantity of sulphur, converted into sulphuric acid by the preceding operations, and precipitated by barytes, be calculated according to the accurate experiments of Mr. CHENEVIX, these 155 grains of sulphate of barytes will denote, nearly, 22.50 of sulphur; so that, with the addition of the 14 grains previously obtained in substance, the total quantity will amount to 36.50.

E. Moreover, from what has been stated it appears, that the iron which was obtained in the form of black oxide, weighed 80 grains; and, by adding these 80 grains to the 36.50 of sulphur,

an increase of weight is found = 16.50. This was evidently owing to the oxidizement of the iron, which, in the magnetical pyrites, exists quite, or very nearly, in the metallic state, but, by the operations of the analysis, had received this addition. The real quantity of iron must, on this account, be estimated at 63.50.

One hundred grains, therefore, of the magnetical pyrites, yielded,

$$\begin{array}{lll} \text{Sulphur}\left\{\begin{array}{ll}\text{A.} & 14 \\ \text{D.} & 22.50\end{array}\right\} & 36.50 & \text{grains.} \\ \text{Iron} \quad \text{E.} \quad = & \underline{63.50} & \\ & 100. & \end{array}$$

This analysis was repeated in a similar manner, excepting that the whole was digested in nitric acid, until the sulphur was intirely converted into sulphuric acid. To the liquor which remained after the separation of the iron by ammonia, muriate of barytes was added, as before, and formed a precipitate which weighed 245 grains. Now, as the sulphuric acid in sulphate of barytes is estimated by Mr. CHENEVIX at 23.5 *per cent.* and the sulphur which is required to form the sulphuric acid contained in 100 parts of sulphate of barytes, at 14.5,* it follows, that 245 grains of dry sulphate of barytes, contain sulphuric acid equal, very nearly, to 36 grains of sulphur; so that the two analyses corroborate each other. The proportion of sulphur in the magnetical pyrites, may therefore be stated at 36.50, or indeed at 37 *per cent.* if some small allowance be made for the occasional presence of earthy particles; a minute portion of quartz having been found, by the last analysis, after the complete acidification of the sulphur.

* Transactions of the Royal Irish Academy, Vol. VIII. p. 240.

The increase produced, by the operations of the analysis, in the weight of the iron, arose, as I have already remarked, from the addition of oxygen; for the iron, as obtained by the analysis, was in the state of black oxide; but in this, and indeed in all pyrites, it undoubtedly exists very nearly, or quite, in the state of perfect metal. Now the black oxide of iron, called Protoxide by Dr. THOMSON,* has been proved, by LAVOISIER and PROUST, to consist of 100 parts of metallic iron combined with 37 of oxygen, thus forming 137 of black oxide; the exact proportion of oxygen is therefore 27 *per cent.* and 80 grains of this oxide must contain 21.6 of oxygen. But, in the above analyses of the magnetical pyrites, the increase of weight did not amount to more than 16.5; and we may therefore conclude that, in all probability, a quantity of oxygen = 5.1 was previously combined with some part, or with the general mass, of the iron in the pyrites. A small part of the abovementioned increase of weight, must likewise have arisen from another cause; for, although the true proportions of the black oxide of iron are 27 of oxygen and 73 of iron, (so that 100 parts of the latter absorb 37 of the former,) yet, in actual practice, it is difficult to obtain it exactly in this state, and there is commonly a small excess of weight: this I have repeatedly observed, in many experiments, some of which were purposely made. When, for instance, 100 parts of fine iron wire were dissolved in muriatic acid, and afterwards precipitated by ammonia, edulcorated, dried, and made red-hot with a small quantity of wax in a covered porcelain crucible, the weight, instead of 137, usually amounted to 139 or 140. The quantity of wax employed, certainly did not afford a ponderable quantity of coal, or other residuum; but the real cause of the increase of

* System of Chemistry, 2d edition, Vol. I. p. 147.

weight, appears to be the air, which can scarcely be completely excluded, and which, after the wax is burned, combines with the superficial part of the oxide, and converts a portion of it into the red or peroxide; so that the surface in the crucible appears brown, when compared with the interior.

To this cause, therefore, I am inclined also to attribute a small part of the increase observed in the weight of the iron obtained by the preceding analyses.

§ V.

Before I make any observations on the nature of the sulphuret which has been proved to constitute the magnetical pyrites, it may be proper to state some comparative analyses which I have made, of several of the common pyrites; and, as the method employed was precisely the same as that which has been described, all that seems to be requisite, is to give an account of the results.

In each analysis, the whole of the sulphur was converted into sulphuric acid, which was precipitated by barytes; and, in the selection of the specimens, great attention was paid, to take the internal parts of the fragments, and not to make use of any which exhibited an appearance of decomposition, or of extraneous substances.

The iron was, as before, reduced to the state of black oxide; and the addition of weight in each separate analysis, corresponded, within a few fractional parts, with the proportion of oxygen requisite to form into black oxide a given quantity of metallic iron, equal to that which in each pyrites was ascertained to be the real proportion, by deducting the quantity of sulphur from the total quantity of each pyrites.

The iron, therefore, in these is completely metallic, and as such is stated in the following results.

No. 1. Pyrites in the form of dodecaedrons with pentagonal faces. - Specific gravity 4830.	Sulphur	52.15
	Iron	47.85
		100.
No. 2. Pyrites in the form of striated cubes.	Sulphur	52.50
	Iron	47.50
		100.
No. 3. Pyrites in the form of smooth polished cubes, found in the lapis ollaris which accompanies the magnetical pyrites. Specific gravity 4831.	Sulphur	52.70
	Iron	47.30
		100.
No. 4. Radiated pyrites. - - - Specific gravity 4698.	Sulphur	53.60
	Iron	46.40
		100.
No. 5. A smaller variety of radiated pyrites. Specific gravity 4775.	Sulphur	54.34
	Iron	45.66
		100.

Considering the difference in the figure, lustre, and colour of these pyrites, I expected to have found a much greater difference in the proportions of their component ingredients; but, as the results are the average of several experiments, I have not any reason to doubt their accuracy.

The pyrites crystallized in regular figures, such as cubes and dodecaedrons, according to the above analyses, contain less sulphur, and more iron, than the radiated pyrites, and perhaps than others which are not regularly crystallized. This difference, however, is not considerable; for the dodecaedral pyrites, which afforded the smallest quantity of sulphur of any of the regularly crystallized pyrites, yielded 52.15; and the radiated pyrites, No. 5, gave 54.34; the difference, therefore, is only 2.19. So that the mean proportion of sulphur, in all the pyrites which were examined, is 53.24 *per cent.* and, taking the proportion of sulphur in the magnetical pyrites at 36.50 or 37, the difference between this and the mean of the common pyrites will be 16.74 or 16.24. The magnetical pyrites, therefore, is quite distinct, as a sulphuret of iron, from the common martial pyrites; and, in the following observations I shall prove, that a sulphuret consisting of the proportions last mentioned, has till now been unknown as a product of nature.

§ VI.

Although pyrites is one of the most common of mineral substances, yet the discovery of its real nature is comparatively of a late date; for it appears, that even AGRICOLA (whose knowledge of mineral bodies was certainly great, considering the state of science in his time) was not acquainted with its characteristic ingredient, namely, *iron*. According to HENCKEL, this was first noticed by our countryman MARTIN LISTER, a member of this learned Society, who says "*Pyrites purus putus ferri metallum* "*est.*"

From the time of HENCKEL, pyrites seems little to have attracted the notice of chemists, until Mr. PROUST, the learned

professor of chemistry at Madrid, published two memoirs, in which he states, that there are two sulphurets of iron, the one being artificial, and the other natural. The first is the sulphuret which is formed in laboratories, by adding sulphur to red-hot iron, or by exposing both of them to heat in a retort. This is distinguished from the second sulphuret, (which is the common martial pyrites,) by its easy solubility in acids, especially in muriatic acid, by the formation of sulphuretted hydrogen gas during the solution of the sulphuret in the last named acid, by its colour, and by its inferior density.

According to Mr. PROUST, the first or artificial sulphuret is composed of 60 parts of sulphur, combined with 100 parts of iron; whilst the second sulphuret, or common pyrites, consists of 90 parts of sulphur and 100 of iron.

He moreover observes, that the sulphur of the first sulphuret is difficultly separated; but that the excess which is in the second sulphuret, or common pyrites, is easily expelled, and is that portion which is obtained by distillation, the residuum being then reduced to the state of the first sulphuret.* 100 parts, therefore, of this substance, are composed of 62.50 of iron and 37.50 of sulphur; and 100 parts of common pyrites are, according to this statement, composed of 52.64 of iron and 47.36 of sulphur.

These proportions, Mr. PROUST considers as the minimum and maximum of the sulphurets of iron. For the latter, he allows

* *Journal de Physique*, Tome LIII. p. 89, and Tome LIV. p. 89. From pp. 91 and 92 of Tome LIV. it is evident, that the author does not mean to assert, that the first sulphuret contains 60 *per cent.* of sulphur; but that 100 parts of iron are combined with 60 of sulphur, and form 160 of the sulphuret. In like manner, when 90 of sulphur are united with 100 of iron, a substance analogous to common pyrites is formed, which weighs 190 grains or parts.

some variation; but the composition of the former, he regards as fixed by the invariable law of proportions;* although he observes, that *it has not as yet been discovered in the mineral kingdom.*†

In support of these assertions, Mr. PROUST states,

1. That the pyrites found near Soria, when distilled in a retort heated to redness, afforded nearly 20 *per cent.* of sulphur.

2. That the residuum of the above distillation, had lost the external characters and chemical properties of pyrites, and had assumed those of the artificial sulphuret of iron.

3. That when to this residuum a quantity of sulphur was added, and the whole was distilled in a degree of heat not too great, the 20 *per cent.* of sulphur, which had been separated by the first distillation, was, by this, again restored; and the mass in the retort thus recovered nearly the original colour, lustre, and chemical properties of the pyrites.

4. That, by adding sulphur to iron filings, or fine iron wire, heated to a low red in a retort, a compound is obtained, in which the proportion of sulphur amounts only to about 20 or 30 parts; but, if this compound is again treated with sulphur in a red heat, a sulphuret is formed, which is readily dissolved in acids, and plentifully affords sulphuretted hydrogen gas.

This is the real minimum of the sulphurets of iron, fixed by the invariable law of proportions, (according to Mr. PROUST,) at 59 or 60 of sulphur and 100 of iron, the former being (as I have already observed) in the proportion of 37.50 *per cent.*

5, and lastly. That when this sulphuret is again mixed and

* *Journal de Physique,* Tome LIII. p. 90.

† "La regne minéral, jusqu'ici, ne nous a point encore présenté le fer sulfuré au "minimum." *Journal de Physique,* Tome LIV. p. 93.

distilled with sulphur, (due attention being paid to the degree of heat,) the product is found to have assumed most of the chemical and external properties of the natural common pyrites, density alone being excepted.

The application of the above observations, to the principal subject of the present Paper, is sufficiently obvious; for, when it is considered, that the magnetical pyrites is so different from the common pyrites, in colour, hardness, solubility in sulphuric acid, and more especially in muriatic acid, with the copious production of sulphuretted hydrogen gas; when, by analysis, it has been found to consist of 36 or 37 of sulphur, combined with about 63 of metallic iron; and, when the artificial sulphuret of iron which has been lately described, is proved to agree with the magnetical pyrites in the nature and proportions of its component ingredients, and in every one of the abovementioned properties; it is evident that the magnetical pyrites is identically the same with this sulphuret, which hitherto has remained undiscovered in nature, and has only been known as a product of our laboratories. In order however more fully to satisfy myself, I made experiments on the artificial sulphuret, which I formed with sulphur and fine iron wire.

This substance agreed, in all the properties which have been noticed, with the magnetical pyrites; and the precipitates obtained by adding prussiate of potash, and ammonia, to the muriatic and sulphuric solutions, were precisely similar. The specific gravity was 4390, whilst (as I have already remarked) that of the magnetical pyrites is 4518.

§ VII.

So far, therefore, as can be proved by similarity in chemical properties and analysis, the magnetical pyrites is indisputably a natural sulphuret, completely the same with that which till now has been only known as an artificial product; but, that the mind may be perfectly satisfied, another question must be solved, namely, how far do they accord in receiving and retaining the property of magnetism? Common pyrites do not appear to affect the magnetic needle, or, if some of them slightly act by attraction, (which however I never could perceive, nor recollect to have read in works expressly relating to magnetism,) yet they do not possess, nor appear capable of acquiring, any magnetic polarity. As, therefore, the iron of pyrites is undoubtedly in the metallic state, and in a considerable proportion, the destruction of this characteristic property of metallic iron, must be ascribed to the other ingredient, sulphur.

But we have lately seen, that a natural combination of iron with 36.50 or 37 *per cent.* of sulphur, is in possession of all the properties supposed hitherto to appertain (in any marked degree) almost exclusively to the well known magnetic iron ore; and that the combination alluded to is strictly chemical, and not (as at first might have been imagined) a mixture of particles of magnetic iron ore with common pyrites.*

This is certainly very remarkable; and it induced me to examine the effects produced by sulphur, on the capacity of metallic

* This has been sufficiently proved, by the facts which have been stated; I shall however add, that upon digesting a mixture of the powder of common pyrites and iron filings in muriatic acid, I only obtained hydrogen gas, exactly as if I had employed the iron filings without the pyrites.

iron for receiving and retaining the magnetic properties. I therefore prepared some sulphuret of iron, by adding a large quantity of sulphur to fine iron wire, in a moderate red heat.

The internal colour and lustre of the product, were not very unlike those of the magnetical pyrites; and, after the mass had been placed during a few hours between magnetical bars, I found that it possessed so strong a degree of polarity, as to attract or repel the needle completely round upon its pivot; and, although several weeks have elapsed since it has been removed from the magnetical bars, it still retains its power, with little diminution; like the magnetical pyrites, however, in its natural state, it is not sufficiently powerful to attract and take up iron filings.

But this sulphuret did not contain so much sulphur as the magnetical pyrites; I therefore mixed some of it, reduced to powder, with a large quantity of sulphur, and subjected it to distillation in a retort, which was at length heated until the intire bulb became red.

The sulphuret, by this operation, had assumed very much the appearance of the powder of common pyrites, in respect to colour; but, in its chemical properties, such as solubility in muriatic acid, with the production of sulphuretted hydrogen gas, as well as in the nature of the precipitates it afforded with prussiate of potash and with ammonia, it perfectly resembled the magnetical pyrites. Moreover, by analysis, it was found to consist of 35 parts of sulphur and 65 of iron; and although (being in a pulverulent state) its power, as to receiving and retaining the magnetic property, could not so easily be examined, yet, by being powerfully attracted by the magnet, with some

other circumstances, there was every reason to conclude, that in this respect also it was not inferior.

Another portion of sulphuret was formed, as above described; it was placed between magnetical bars, and, in like manner, received and retained the magnetic power.

It is certain, therefore, that when a quantity of sulphur equal to 35 or 37 *per cent.* is combined with iron, it not only does not prevent the iron from receiving the magnetic fluid, but enables it to retain it, so that the mass acts in every respect as a permanent magnet.

Black oxide of iron, by one operation, does not appear to combine with sulphur so readily as iron filings; a second operation, however, converts it into a sulphuret, very much resembling that which has just been described, including the chemical as well as the magnetical properties; but, undoubtedly, by these processes, it is progressively converted, perfectly or very nearly, into the metallic state.

Iron combined with a larger proportion of oxygen, such as the fine gray specular iron from Sweden, will not form a sulphuret by the direct application of sulphur, in one operation; although it becomes of a dark brown colour, partly iridescent, and is moderately attracted by a magnet.

50 grains of the magnetical pyrites, reduced to powder, and mixed with three times the weight of sulphur, were distilled in a retort, until the bulb became moderately red-hot. After the distillation, the pyrites weighed 54.50; consequently, the addition of sulphur was 9 *per cent.* making the total =45.50 or 46 *per cent.* The powder was become greenish-yellow, very like that of the common pyrites: it did not afford any sulphuretted

hydrogen, when digested in muriatic acid; but it nevertheless was partially dissolved, and the solution, when examined by prussiate of potash, and by ammonia, was not different from that of the crude magnetical pyrites.

The powder which had been distilled with sulphur, and which had thus received an addition of 9 *per cent.* to its original quantity, *was still capable of being completely taken up by a magnet.*

From the whole of the experiments which have been related, it is therefore evident, that iron, when combined with a considerable proportion of sulphur, is not only still capable of receiving the magnetic property, but is also thereby enabled to retain it, and thus (as I have already remarked) becomes a complete magnet; and it is not a little curious, that iron combined (as above stated) with 45 or 46 *per cent.* of sulphur, is capable of being taken up by a magnet, whilst iron combined with 52 *per cent.* or more, of sulphur, (although likewise in the metallic state,) does not sensibly affect the magnetic needle; and hence, small as the difference may appear, there is reason to conclude, that the capacity of iron for magnetic action is destroyed by a certain proportion of sulphur, the effects of which, although little if at all sensible at 46 *per cent.* are yet nearly or quite absolute, in this destruction of magnetic influence, before it amounts to 52. But, what the exact intermediate proportion of sulphur may be, which is adequate to produce this effect, I have not as yet determined by actual experiment.

As carbon acts on soft iron, (which, although it most readily receives the magnetic influence, is unable to retain it so as to become a magnet, without the addition of a certain proportion

of carbon, by which it is rendered hard and brittle, or, in other words, is converted into steel,) so, in like manner, does sulphur seem to act; for it has been proved, by the preceding experiments, that the brittle mass formed by the union of a certain proportion of this substance with iron, whether by nature or by art, becomes capable of retaining the magnetic virtue, and of acting as a complete magnet.

This remarkable coincidence, in the effects produced on iron by carbon and sulphur, induced me to try the effects of phosphorus; and my hope of success was increased by the remark of Mr. PELLETIER, who says, that "the phosphuret of iron is attracted by the magnet;"* and therefore, although certain bodies may be thus attracted, without being capable of actually becoming permanent magnets, I was desirous to examine what might be the power, in this respect, of phosphuret of iron.

I therefore prepared a quantity of phosphuret of iron, in the direct way, *viz.* by adding phosphorus, cut into small pieces, to fine iron wire made moderately red-hot in a crucible. The usual phenomena took place, such as the brilliant white flame, and the rapid melting of the iron, which, when cold, was white, with a striated grain, extremely brittle, hard, and completely converted into a phosphuret. The fragments of this were powerfully attracted by a magnet; and, after I had placed two or three of the largest pieces, during a few hours, between magnetical bars, I had the pleasure to find that these had become powerful magnets, which not only attracted or repelled the needle completely round, but were able to take up iron filings, and small

* "Le Phosphure de Fer est attirable a l'aimant." *Annales de Chimie,* Tome XIII. p. 114.

pieces, about half an inch in length, of fine harpsichord wire; and, although they have now been removed from the magnetical bars more than three weeks, I cannot discover any diminution of the power which had thus been communicated to them.

The three inflammable substances, *carbon, sulphur,* and *phosphorus,* which, by their chemical effects on iron, in many respects resemble each other, have now therefore been proved alike to possess the property of enabling iron to retain the power of magnetism; but I shall consider this more fully in the following section.

§ VIII.

From the whole which has been stated we find,

1. That the substance called magnetical pyrites, which has hitherto been found only in Saxony and a few other places, is also a British mineral, and that, in Caernarvonshire, it forms a vein of considerable extent, breadth, and depth.

2. That the component ingredients of it are sulphur and metallic iron; the former being in the proportion of 36.50 or 37, and the latter about 63.50 or 63.

3. That the chemical and other properties of this substance are very different from those of the common martial pyrites, which however are also composed of sulphur and iron, varying in proportion, from 52.15 to 54.34 of sulphur, and from 47.85 to 45.66 of metallic iron; the difference between the common pyrites which were examined being therefore 2.19, and the mean proportions amounting to 53.24 of sulphur, and 46.75 of iron; consequently, the difference between the relative proportions, in

the composition of the magnetical pyrites and of the common pyrites, is nearly 16.74, or 16.24.

4. That, as the magnetical pyrites agrees in analytical results, as well as in all chemical and other properties, with that sulphuret of iron which hitherto has been only known as an artificial product, there is no doubt but that it is identically the same; and we may conclude, that its proportions are most probably subjected to a certain law, (as Mr. PROUST has observed in the case of the artificial sulphuret,) which law, under certain circumstances, and especially during the natural formation of this substance in the humid way, may be supposed to act in an almost invariable manner.

5. That, in the formation of common martial pyrites, there is a deviation from this law, and that sulphur becomes the predominant ingredient, which is variable in quantity, but which, by the present experiments, has not been found to exceed 54.34 *per cent.* a proportion, however, that possibly may be surpassed in other pyrites, which have not as yet been chemically examined.

6. That iron, when combined naturally or artificially with 36.50 or 37 of sulphur, is not only still capable of receiving the magnetic fluid, but is also rendered capable of retaining it, so as to become in every respect a permanent magnet; and the same may, in a great measure, be inferred respecting iron which has been artificially combined with 45.50 *per cent.* of sulphur.

7. That, beyond this proportion of 45.50 or 46 *per cent.* of sulphur, (in the natural common pyrites,) all susceptibility of the magnetic influence appears to be destroyed; and, although the precise proportion which is capable of producing this effect, has not as yet been determined by actual experiment, it is

certain that the limits are between 45.50 and 52.15; unless some unknown alteration has taken place in the state of the sulphur, or of the iron, in the common martial pyrites.

8. That, as carbon, when combined in a certain proportion with iron, (forming steel,) enables it to become a permanent magnet, and as a certain proportion of sulphur communicates the same quality to iron, so also were found to be the effects of phosphorus; for the phosphuret of iron, in this respect, was by much the most powerful, at least when considered comparatively with sulphuret of iron.

9, and lastly, that as carbon, sulphur, and phosphorus, produce, by their union with iron, many chemical effects of much similarity, so do each of them, when combined with that metal in certain proportions, not only permit it to receive, but also give it the peculiar power of retaining, the magnetical properties; and thus, henceforth, in addition to that carburet of iron called steel, certain sulphurets and phosphurets of iron may be regarded as bodies peculiarly susceptible of strong magnetical impregnation.

Having thus, for the greater perspicuity, reduced the principal facts of this Paper into a concise order, I shall now make some general observations.

It is undoubtedly not a little singular, that a substance like the magnetical pyrites, which, although not common, has been long known to mineralogists, should not hitherto have been chemically examined, especially as mineralogical authors have mentioned the analysis of it as a desideratum. The result of this which I have attempted, proves that it is really deserving of notice; for thus we have ascertained, that the sulphuret of

iron hitherto known only as an artificial product, is also formed by nature; and that the composition of this last, agrees with those proportions of the artificial sulphuret which have been stated by Mr. PROUST.

But, from this sulphuret or magnetical pyrites, I have not, by analysis, as yet been able to discover any regular or immediate gradations into the common pyrites; for the least proportion of sulphur in these amounted to 52.15, and the greatest proportion to 54.34; so that, between the magnetical and the common pyrites, the difference is considerable, in the proportions of their component substances, as well as in their physical and chemical properties; whilst the difference which I have hitherto been able to detect in the proportions of some of the common pyrites, (very dissimilar in figure, lustre, colour, and hardness,) has only amounted to 2.19.

Mr. PROUST, in a general way, considers common pyrites to differ from the first sulphuret, or that composed of 60 parts of sulphur and 100 of iron, (= 37.50 *per cent.*) by containing a farther addition of half the above quantity of sulphur, or 90 parts of sulphur and 100 of iron, (= 47.36 *per cent.*) but this opinion he appears to have formed, in consequence of results obtained by synthetical experiments made in the dry way. Now, when we consider how difficult it is to regulate the high degrees of temperature, and what a numerous chain of alterations in the relative order of affinities most commonly result from alterations in these degrees of heat, it seems to me that we cannot rely, with absolute certainty, on synthetical experiments made in the above way, unless they are corrected, and contrasted with analytical experiments made on the same substances. But it does

not appear, from the two memoirs published by Mr. Proust, to which I have so frequently alluded, that that gentleman did more, in respect to analysis, than distil the cubic and dodecaedral pyrites found near Soria, from which he obtained about 20 *per cent.* of sulphur; and, having observed that the residuum possessed the properties of the sulphuret which has been commonly prepared in laboratories, he concluded that the sulphur obtained from the pyrites, is the excess of that proportion which is requisite to form the sulphuret, the proportions of which, therefore, he by synthesis ascertained to be, as I have above stated, = 37.50 of sulphur, and 62.50 of iron, or 60 of sulphur combined with 100 of iron; and lastly, having formed 318 grains of this sulphuret from 200 grains of iron filings, he distilled the sulphuret with an additional quantity of sulphur, in an inferior degree of heat, and obtained 378 grains of a substance which, excepting density, was similar to the common martial pyrites.*

It is however to be regretted, that Mr. Proust did not make a regular analysis of the pyrites of Soria, and of the residuum after distillation; for (unless these pyrites are very different from those which I have examined) he would most probably have found the proportion of sulphur greater than that which he has assigned to natural pyrites in general. This at least there is great reason to suppose, if we allow that most or all of the pyrites have been formed in the humid way, by which, we may conceive, a larger proportion of sulphur may be introduced into the compound, than can take place in high degrees of temperature. And this opinion is corroborated by the results of

* *Journal de Physique*, Tome LIV. p. 92.

my analyses; for, instead of finding the general proportions to be 47.36 of sulphur and 52.64 of iron, the mean result of these analyses is very nearly the reverse, being 53.24 of sulphur and 46.76 of iron.

Mr. PROUST is also of opinion, that the pyrites which contain the smallest quantity of sulphur, are those which are most liable to vitriolization; and, on the contrary, that those which contain the largest proportion, are the least affected by the air or weather.* This opinion of the learned professor, by no means accords with such observations as I have been able to make; for the cubic, dodecaedral, and other regularly crystallized pyrites, are liable to oxidizement, so as to become what are called hepatic iron ores, but not to vitriolization; whilst the radiated pyrites (at least those of this country) are by much the most subject to the latter effect; and therefore, as the results of the preceding analyses show that the crystallized pyrites contain less sulphur than the radiated pyrites, I might be induced to adopt the contrary opinion. But I am inclined to attribute the effect of vitriolization observed in some of the pyrites, not so much to the proportion, as to the state of the sulphur in the compound; for I much suspect, that a predisposition to vitriolization, in these pyrites, is produced by a small portion of oxygen being previously combined with a part, or with the general mass, of the sulphur, at the time of the original formation of these substances, so that the state of the sulphur is tending to that of oxide, and thus the accession of a farther addition of oxygen becomes facilitated. We have an example of similar effects in phosphorus, when (as is commonly said) it is half burned, for the

* *Journal de Physique.* Tome LIII. p. 91.

purpose of preparing the phosphorus bottles; and the propensity to vitriolization, observed in many of the half-roasted sulphureous ores, appears to me to arise from this cause, rather than from the mere diminution of the original proportion of sulphur, or the actual immediate conversion of part of it into sulphuric acid; nevertheless, I offer this opinion, at present, only as a probable conjecture, which may be investigated by future experiments and observations.

The magnetical properties of the sulphuret of iron which forms the principal subject of this Paper, must be regarded as a remarkable fact; for I have not found, in the various publications on magnetism which I have had the means of consulting, even the most remote hint, that iron when combined with sulphur, is possessed of the power of receiving and retaining the magnetic fluid; and, judging by the properties of common pyrites, we might have supposed that sulphur annihilated this power in iron, as indeed seems to have been the opinion of mineralogists, who have never enumerated magnetical attraction amongst the physical properties of those bodies; and, although Werner, Widenmann, Emmerling, and Brochant, have arranged the magnetical pyrites with the sulphurets of iron, yet the magnetical property could not with certainty be stated as inherent in the sulphuret, for, at that time, this substance had not been subjected to a regular chemical analysis, and the magnetical property might therefore be suspected to arise from interspersed particles of the common magnetical iron ore. This probably has been the opinion of the Abbé Haüy; for, in his extensive Treatise on Mineralogy lately published, I cannot find any mention made of the magnetical pyrites, either amongst the sulphurets or amongst the other ores of iron.

In the mineral kingdom, a great variety of substances, and even some of the gems, exert a feeble degree of attraction on the magnetic needle, and sometimes also acquire a slight degree of polarity;* but, as this wonderful property has only been observed conspicuously powerful in one species of iron ore, this has been always emphatically called *the Magnet*,† and is said to consist of metallic iron combined with from 10 to 20 *per cent.* of oxygen.

From the facts, however, which have been recently stated, we now find that there is another natural substance, apparently very different from the magnet in chemical composition, but nevertheless approaching very nearly to it in power, which is found in several parts of our globe, and particularly in a province of this kingdom, where it constitutes a vein, running north and south, of considerable extent, and several yards in width and thickness.

From the experiments also, which have been made on the artificial preparation of this substance, we find, that it is capable of receiving the magnetic properties when the proportion of sulphur amounts to 37 *per cent.* and is still powerfully attracted when a much larger quantity of sulphur is present. There is, however, some point at which all these effects cease, and this point appears to be, when the sulphur is in some proportion between 45 or 46 and 52 *per cent.* The preceding experiments have also proved, that iron when combined with phosphorus, likewise possesses the power of becoming a magnet to a very remarkable degree; and, by the similarity, in this respect, of the

* CAVALLO on Magnetism, page 73.

† In a future Paper, it is my intention to give an account of some comparative analyses of the varieties of this substance.

carburet of iron called steel, to the above sulphuret and phosphuret, a very remarkable analogy is established between the effects produced on iron, by carbon, sulphur, and phosphorus.

Carbon, when combined in a very large proportion with iron, forms the carburet of that metal, called plumbago; a brittle substance, insoluble in muriatic acid, and destitute of magnetical properties. But, smaller proportions of carbon, with the same metal, constitute the various carburets included between black cast iron and soft cast steel;* bodies which are more or less brittle, soluble in muriatic acid, and more or less susceptible of magnetical impregnation; some of them form the most powerful magnets hitherto discovered.

Sulphur, in like manner, combines with iron in a large proportion, forming the common pyrites, which are brittle, almost or quite insoluble in muriatic acid, and devoid of magnetical properties. Sulphur in smaller proportions, forms sulphurets

* "When the carbon exceeds, the compound is carburet of iron or plumbago: when "the iron exceeds, the compound is steel, or cast iron, in various states, according to "the proportion. All these compounds may be considered as subcarburets of iron." THOMSON's System of Chemistry, Vol. I. p. 165.

Mr. MUSHET, in the following Table, exhibits the proportion of charcoal which disappeared, during the conversion of iron to the different varieties of subcarburet known in commerce.

"Charcoal absorbed.	Result.
$\frac{1}{120}$ - - -	Soft cast steel.
$\frac{1}{100}$ - -	Common cast steel.
$\frac{1}{90}$ - - -	The same, but harder.
$\frac{1}{50}$ - -	The same, too hard for drawing.
$\frac{1}{25}$ - - -	White cast iron.
$\frac{1}{20}$ - - -	Mottled cast iron.
$\frac{1}{15}$ - - -	Black cast iron.

"When the carbon amounts to about $\frac{1}{60}$ of the whole mass, the hardness is at the maxi-"mum." THOMSON, Vol. I. p. 166; and Phil. Magazine, Vol. XIII. pp. 142 and 148.

which are also brittle, but are soluble in muriatic acid, and strongly susceptible of magnetical impregnation.

Phosphorus also, when combined with iron, makes it brittle, and enables it powerfully to receive and retain the magnetical properties; so that, considering the great similarity which prevails in other respects, it may not seem rash to conclude, that phosphorus, (like carbon and sulphur,) when combined with iron in a very large proportion, may form a substance incapable of becoming magnetical, although, in smaller proportions, (as we have seen,) it constitutes compounds which are not only capable of receiving, but also of retaining, the magnetical properties, even so far as, in some cases, to seem likely to form magnets of great power; and, speaking generally of the carburets, sulphurets, and phosphurets of iron, I have no doubt but that, by accurate experiments, we shall find that a certain proportion of the ingredients of each, constitutes a maximum in the magnetical power of these three bodies. When this maximum has been ascertained, it would be proper to compare the relative magnetical power of steel (which hitherto has alone been employed to form artificial magnets) with that of sulphuret and phosphuret of iron; each being first examined in the form of a single mass or bar of equal weight, and afterwards in the state of compound magnets, formed like the large horse-shoe magnets, by the separate arrangement of an equal number of bars of the same substance in a box of brass.

The effects of the above compound magnets should then be tried against others, composed of bars of the three different substances, various in number, and in the mode of arrangement; and, lastly, it would be interesting to make a series of experiments on chemical compounds, formed by uniting different proportions of

carbon, sulphur, and phosphorus, with one and the same mass of iron. These quadruple compounds, which, according to the modern chemical nomenclature, may be called carburo-sulphuro phosphurets, or phosphuro-sulphuro-carburets, &c. of iron, are as yet unknown as to their chemical properties, and may also, by the investigation of their magnetical properties, afford some curious results. At any rate, an unexplored field of extensive research appears to be opened, which possibly may furnish important additions to the history of magnetism, a branch of science which of late years has been but little augmented, and which, amidst the present rapid progress of human knowledge, remains immersed in considerable obscurity.

XIII. *Remarks on the voluntary Expansion of the Skin of the Neck, in the Cobra de Capello or hooded Snake of the East Indies. By* Patrick Russell, *M. D. F. R. S. With a Description of the Structure of the Parts which perform that Office. By* Everard Home, *Esq. F. R. S.*

Read June 14, 1804.

The remarkable expansion of the skin of the neck, in the Coluber Naja of Linnæus, or Cobra de Capello of the East Indies, and which constitutes a principal character of the species, is produced by an apparatus hitherto, as I believe, very imperfectly described. It is a voluntary action, totally distinct from that inflation which all serpents, when irritated, are more or less capable of, and which the Coluber Naja also assumes, at the same time that it expands its hood.

In botanical excursions in India, fragments of serpentine skeletons, made by the black ants, were occasionally met with; but, in such as were supposed to belong to the Coluber Naja, the peculiar disposition and structure of the cervical ribs, so different from that in other serpents, had escaped me.

In other serpents, the ribs, from the first vertebra to those of the middle of the trunk, gradually increase in length; thence they gradually shorten or decline, to near the end of the tail, where they disappear, or are transformed into short eminences; but, in the Coluber Naja, the cervical ribs gradually lengthen to the tenth or eleventh, after which, they successively shorten to the twentieth. The ribs, again increasing in length, are, at the

middle of the trunk, nearly as long as the middle cervical ribs; and then declining, as usual in other serpents, disappear on the tail.

So obvious a peculiarity in the skeleton of the Cobra de Capello having escaped my notice in India, and finding myself unable to account for the expansion of its hood, which is commonly, in that country, conceived to be connected with inspiration, I brought with me, on my return to England, several subjects for dissection, in order to have the matter properly ascertained. My friend Mr. Home readily undertook the task; and the subjoined result of his investigation will, I have no doubt, prove satisfactory.

I have, on another occasion, asserted as a fact, that the neck of the Cobra de Capello, in a quiescent state, shows no external protuberance whatever; * and it is clearly accounted for, in the following description, from the ribs, when depressed, lying upon the spine, over one another.

Mr. Home's *Description.*

The mechanism by which the Cobra de Capello, when irritated and ready to seize its prey, expands the skin of the neck, giving it the appearance from which the snake takes its name, consists intirely of muscles, acting upon the ribs and external skin of the animal.

From the rounded form of the hood, the skin has the appearance of being inflated; but the most careful examination did not discover any communication between the trachea, or the lungs, and the cellular membrane under the skin.

In this snake, the ribs nearest the head, to the number of twenty on each side, have a different shape from the rest; instead

* Continuation of an Account of Indian Serpents, page 3. Lond. 1801.

of bending equally with the other ribs towards the belly, they go out in a lateral direction, having only a slight curvature, and, when depressed, lie upon the side of the spine, on one another.

The first rib is shorter than the rest; and they become gradually longer to the tenth and eleventh, which are the longest; they afterwards become gradually shorter to the twentieth, which is nearly of the same length as the first; so that the ribs on each side, when extended, form an oval figure, of which the spine is the middle line or long axis.

In the extended state of the ribs, the skin of the back is brought over them, forming the hood; and, in their depressed state, the hood disappears.

The ribs are raised by four sets of muscles: one set, from the spine to the upper edge of each rib; a second set, from the ribs above, passing over two ribs to the third rib below; another set have their origin from the rib above, pass over one rib, and are inserted into the second below; and a fourth set pass from rib to rib.

The combined effect of these four sets of muscles, raises and extends the ribs: their direction and appearance is so distinctly seen in the annexed Figures, as to make a more particular description, in a Paper of this kind, unnecessary.

The skin of the back is brought forwards on the neck, by a large set of very long muscles, going off from each of the first twenty ribs on each side, a quarter of an inch from their head, by a tendinous origin, which soon becomes fleshy; the longest of these muscles is two inches long; they are inserted into the skin, and, when the ribs have been first extended, have the power of bringing the skin forwards to a great extent.

By these means, the hood is formed.

To depress the ribs, and restore the parts to that state in which the neck of the animal does not appear disproportionally protuberant, but of the same size as the rest of the snake, there are three sets of muscles: one set goes from the vertebræ of the neck to the lower edge of each rib; but, to give these muscles a greater length of fibre, they are not inserted into the rib immediately above the vertebræ, but pass upwards and outwards over three ribs, and are inserted into the fourth, at the middle part of it. These muscles become antagonists to those which raise the ribs.

The second set arises from the points of the ribs; and each muscle goes to be inserted into the skin, nearer the head, counteracting the muscles which bring the skin forwards, and drawing it, by their action, back again. The third set goes from the root of one scutum to the root of the scutum immediately above it, so as to bring it down upon the other.

The object of the present Paper being to explain the mechanism upon which the hood, the peculiar characteristic of this species of snake, depends, it is not meant to enter into the uses for which the hood is intended. It may not however be improper to observe, that the expansion of the ribs answers no good purpose respecting the lungs, since they are not so situated, in this animal, as to receive any advantage from it; but the gullet, where it passes down along the neck, admits of great expansion; and the extended state of the ribs, at the time the animal is employed in catching its prey, may give to the gullet a facility of being dilated, for the reception of the food.

EXPLANATION OF THE FIGURES.

Plate VII.

Fig. 1. A side view of the head and neck of the Cobra de Capello, drawn from the living animal.

Fig. 2. A back view of the hood.

Fig. 3. A front view of the hood.

Plate VIII.

Fig. 4. A back view of the neck, in its expanded state; the external skin being dissected off, and turned aside, to show the muscles which raise the ribs, and bring the skin forwards towards the head.

This view is intended principally to exhibit the muscles which raise the ribs, and those which, when the ribs are raised, act upon the external skin, and bring it forwards.

AA. The scales on the head of the snake.

BB. The eyes.

CC. The muscles which surround the poison glands.

DD. A portion of the poison glands exposed.

EE. A pair of muscles which rise from the neck, and terminate in the head.

F. One of a pair of muscles which bring the head back.

GG. The skin, divided in the middle line of the back, dissected from the muscles, and turned on each side.

HH. The intercostal muscles.

II. The muscles which bring forward the skin of the back upon the neck, to form the hood; they arise from the ribs, and are inserted into the skin.

KK. Muscles which raise the ribs; they originate from that part of the rib near the spine, pass over two ribs, and are inserted into the rib below, near its extremity.

LL. Muscles which raise the ribs, arising from one rib, and passing over the next, to be inserted into the rib below.

MM. The intercostal muscles.

Fig. 5. A front view of the neck; the parts are dissected, to show the mode in which the ribs lie in their depressed state, also the muscles by which they are depressed, and those which bring the skin back into its natural state.

AA. The two portions of the lower jaw, separated from each other, and turned aside.

BB. The poison fangs.

CCC. The ribs in their depressed state, lying over each other, on the side of the spine.

DDD. The ribs on the opposite side, in their extended state; their extremities become the boundary of the hood, and give it an oval form.

EE. A pair of muscles which bring the head forward upon the neck.

FF. The intercostal muscles.

GG. The muscles which bring the ribs downwards upon the spine.

HH. The muscles which bring the skin backwards from the neck; they have their origin from the points of the ribs, and are inserted into the lower edge of the abdominal scuta.

II. The abdominal scuta, divided in the middle line of the belly.

KK. The muscles which go from the lower edge of one scutum to the lower edge of the scutum over it, to bring the scuta closer together, and make them overlap.

LL. An internal view of the skin of the snake, beyond the abdominal scuta.

Fig. 1.

Fig. 2.

Fig. 3.

W. Clift, del.

Fig. 4.

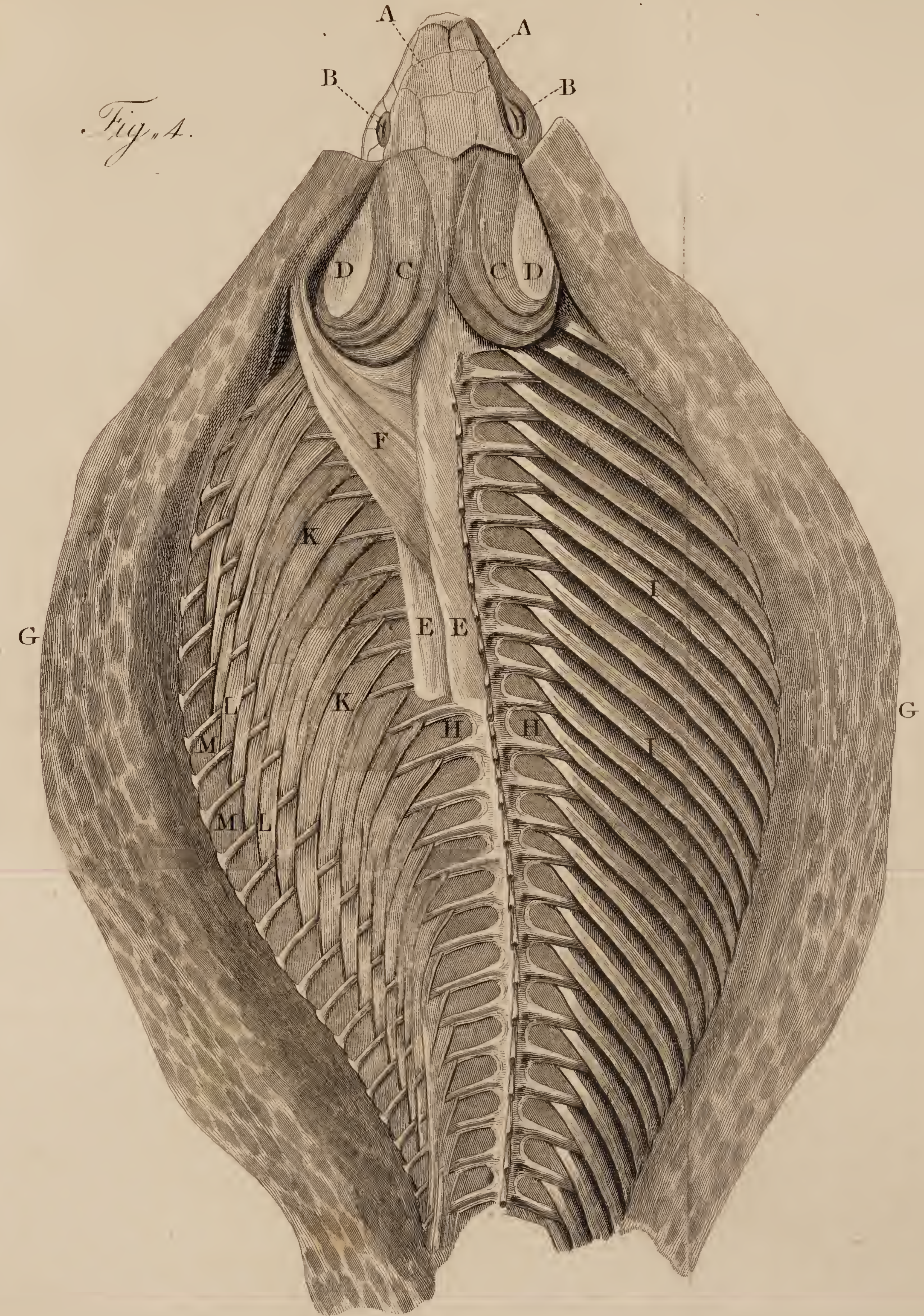
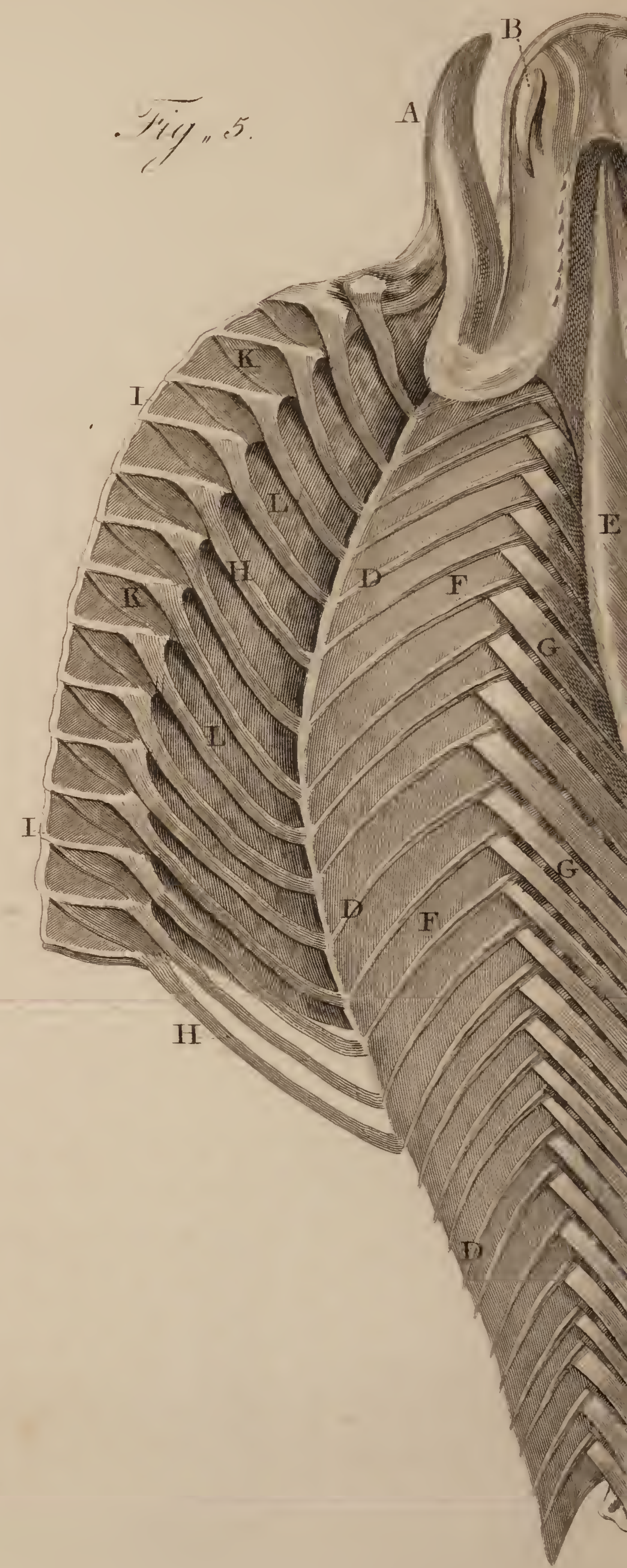


Fig. 5.



XIV. *Continuation of an Account of the Changes that have happened in the relative Situation of double Stars. By* William Herschel, *LL. D. F. R. S.*

Read June 7, 1804.

In my former Paper,* I have given the changes which have happened in the situation of six double stars. When the causes of these observed changes in the double star Castor were investigated, I had recourse to the most authentic observations I could find, of the motions in right-ascension and polar distance of this star. But the Tables which have been lately published, in the last volume of the observations made by the Astronomer Royal at Greenwich, give us now the proper motions of 36 principal stars, of which α Geminorum is one; and, as the motion of this star, especially in north polar distance, is very different from what it has been supposed in my former examination, it will be necessary to review the arguments which have been used, in order to ascertain what will be the result of this new motion. We shall here again follow the order of the paragraphs of the former Paper, and denote those which treat of the same motions, with the same letters, that they may be readily compared.

Single Motions.

(*a*) The small star x cannot be alone in motion, as we have now, in the new Tables I have mentioned, an evident proof that the large star α is not at rest.

* See Phil. Trans. for 1803, p. 339.

(*b*) As the observations of the Astronomer Royal have ascertained the motion of Castor, so it is no less evident, from the series of observations which has been given in my Paper, that its smaller companion has also changed, if not its real situation, at least its relative one with respect to the large star. Let us therefore examine, whether the motion of α can be the cause of the apparent change that has taken place in the relative situation of these two stars.

The annual proper motion of α Geminorum, in right ascension, by the new Tables, is $0'',15$; which, in $23\frac{1}{2}$ years, will amount to $3'',525$. The annual proper motion in polar distance, by the same Tables, is $0'',04$; which, in the same time, will amount to $0'',94$; the former motion being retrograde, and the latter towards the south. Let FP, in Figure 1, (Plate IX.) be the parallel of Castor, and make $\alpha\alpha'$ equal to 2978,5; which will be the quantity of its motion in right ascension in the parallel, when it is 3525 in the equator. At right angles to αP, make $\alpha'\alpha''$ equal to 940; and this will represent the motion of the star in polar distance towards the south. Draw the line αx so as to make an angle of $32°\ 47'$ with the parallel FαP on the north preceding side, and place x at the distance of 3765 from α. Then will α and x be the situation in which these two stars were observed in the year 1779; their apparent distance, estimated in diameters of the large star, being $1\frac{7}{8}$; and the angle of position, as has been stated, $32°\ 47'$ north-preceding.

If the star x had been at rest while α moved towards α'', the relative situation of the two stars in the year 1803 would have been represented by $\alpha'' x$; that is to say, the apparent disks of these two stars would have been hardly $1\frac{1}{3}$ diameter of the largest asunder, and the angle of position $x\alpha''$P' must have

been 86° 25′ north-preceding. But this is quite inconsistent with the observations that have been given; according to which, the small star, in the year 1803, was situated at x'. It is therefore proved, that the motion of α alone cannot account for the change which has taken place.

(*c*) If the motion of Castor should be only an apparent one, arising from the motion of the solar system, then the proper motion of the sun must be just the reverse of that which the new Tables assign to α Geminorum. This being admitted, let us examine what will be the result with regard to the relative situation of the small star, which, since only the sun is supposed to be in motion, must now remain at rest, as well as α. The effect of the parallax, which we are now considering, is inversely as the distances of the stars which are affected by it. Hence arise the three cases which have been examined in my first Paper.

When a line from the sun to Castor, Oα,* is perpendicular to the line αx, joining the two stars, no change in their relative situation can take place, arising from parallax, which will act equally on both. For, let α, α'' and x, in Fig. 2, be placed as they were in Fig. 1; and the real motion of the sun from O to O′, will produce the parallactic motion of Castor from α to α''. It will also occasion an apparent motion of x, equal to that of α, and in a parallel direction with it. This star will therefore appear to have moved from x to x', in the same time that the large star has moved from α to α'', so that their relative situation will remain unchanged.

(*d*) If x be placed beyond α, the effect of parallax, exerted in the direction xx'', parallel to $\alpha\alpha''$, will be less upon this star than on Castor; and its apparent motion must fall short of the

* See Figure 1 of the former Paper.

situation x'. The consequence of this will be an increase of the angle of position; but, as we know, from the observations which have been given, that this angle has been decreasing, it follows, that the small star cannot be admitted to have been at rest, if we place it farther from us than α.

(*e*) When the smallest of the two stars of our double star is supposed to be much nearer than the largest, the effect of parallax will carry it beyond x'. Let its distance from us to that of α be, for instance, as 3123 to 6076. In this case, while α appears to move as far as α'', x will be seen to move to x''; where its angle of position $x'' \alpha''$ P', will be just 10° 53' north-preceding, as by observation it was found to be in the year 1803. But, according to this hypothesis, the distance $\alpha'' x''$ of the two stars, ought now to be nearly double what it was in 1779; and, since this is contrary to observation, we must also give up this last supposition.

Double Motions.

(*f*) Let us now suppose α and x to be in motion, while the solar system remains at rest. Then, since there will be no parallax that can affect the appearance of these motions, they must be real, and proper to each of the stars. But the circumstances that must take place, in order to produce the phenomena which have been observed, are so particular, that we shall soon find the great improbability of such an accidental arrangement of them as would answer the end. It has already been shown, in the paragraph (*c*) of the former Paper, that we cannot place the two stars at an equal distance from us; and it would be the height of improbability to suppose them to move in parallel planes. But, whatever may be the directions and velocities of

the motions of the stars, or at whatsoever different distances we may place them, the effect which is to arise from these combined circumstances is positively determined; for the star α must appear to move at the rate of 3″,123 of an arch of a great circle in $23\frac{1}{2}$ years, and in the direction of 17° 31′ south-preceding its parallel; while the star x, in the same time, must seem to move over an arch of 4″,179, in a direction of 32° 52′ south-preceding the same parallel. When these quantities, resulting from the proper motions of our new Tables, are substituted for those which have been used in the paragraph (*f*) of my former Paper, the arguments which it contains will remain in full force, and need not be here repeated.

(*g*) The same argument which has been used in the first Paper, when the sun and the small star only were supposed to be in motion, will perfectly apply to the proper motion of Castor, as given in the new Tables. For, as this motion is now to be accounted for by the motion of the sun, we have only to substitute the velocity of 3″,123, in a direction which makes an angle of 17° 30′ 56″ north-following with the parallel of α Geminorum, for the quantity of the solar motion before used; and to assign a proper motion to the small star, having a direction of 68° 10′ south-preceding the parallel of α Geminorum, with a velocity which, if the star was at the distance of α from us, would carry it in $23\frac{1}{2}$ years through 1″,4303.

(*h*) When the sun and Castor only are supposed to be in motion, the former statement of the case will in every respect remain conclusive.

Motion of the three Bodies.

(*i*) It remains now only to be shown, that the arguments which are contained in my first Paper, against the probability of

a supposition which ascribes all the observed phenomena to three real motions, will not be affected by the given alteration in the proper motion of Castor. Without repeating any part of the discussion of the former paragraph (*i*), it will be sufficient if I point out three motions, such as will answer the required purpose.

Let the solar motion, as before, be towards λ Herculis, with such a velocity as will in 23½ years produce a parallactic motion, at the distance and situation of α Geminorum, amounting to 2″,2805, in a direction of 60° 36′ 57″ south-preceding the parallel of that star. Let Castor have a real motion, which in 23½ years would carry it over an arch of 2″,1341, in a direction of 29° 23′ 3″ north-preceding its parallel; and let the real motion of the small star be such that in 23½ years, at its distance from us, supposed to be to that of Castor as 3 to 2, it would describe an arch of 2″,9212, in a direction of 18° 50′ 13″ south preceding. Then would the parallactic motion of α,= 2″,2805, compounded with the real motion we have mentioned, give us an apparent annual motion equal to that which, in Dr. MASKELYNE'S Table, is called the proper motion in right ascension and polar distance of this star. And the parallactic motion of x,= 1″,5203, compounded with the real motion we have assigned, would also produce an apparent annual motion which would correspond with my series of observed situations of this small star. But, for the high improbability of such an hypothesis, I refer to the paragraph (*i*) of my former Paper.

What has been said of Castor, will apply to every other double star of which the proper motion may hereafter be assigned; for, unless the parallactic motion arising from the motion of the solar system should completely explain the observed changes, the same arguments will still remain in full force.

I shall now proceed to a continuation of my account of the changes that have happened in the relative situation of double stars, either in their position or their mutual distance; and, in the following list of them, it will be seen that, of 50 changeable double stars which are given, 28 have undergone only moderate alterations, such as do not amount to an angle of 10 degrees. None of them however have been admitted, except where the change was at least so considerable, that the micrometer which was used on this occasion could ascertain the change with a proper degree of accuracy. Two of the stars, indeed, have hardly suffered any alteration in the angle of position; but, with them it will be found, that a change in their distance has been so ascertained as not to admit of any doubt. Thirteen of the stars have altered their situation above 10 degrees, but less than 20. Three stars have undergone a change in the angle of position, of more than 20, and as far as 30 degrees. The six remaining stars afford instances of a still greater change, which, in the angle of position of some of them, amounts to more than 30 degrees; in others, to near 40, 50, 60, and upwards, to 130 degrees.

α *Herculis.* II, 2.*

The two stars of this double star have undergone a considerable change in their angle of position. By a measure taken May 20, 1781, it was 21° 28′ south-following.† April 3, 1783, two measures gave 25° 29′. A mean of two measures, taken

* The numbers after the name of the star, refer to my Catalogues of double Stars, published in the Philosophical Transactions. For instance, II, 2, denotes that α Herculis is the 2d star in the 2d class.

† By mistake, the first angle of position in my Catalogue is given 30° 35′, instead of 21° 28′, and should be corrected. See Phil. Trans. Vol. LXXII. Part I. p. 122.

Feb. 21 and March 4, 1802, was 31° 38′. By five measures, taken in 1803 and the beginning of 1804, it was 31° 54′; and, June 3, 1804, by a very accurate measure, with an improved illumination of the wires, it was 32° 50′. This gives a change of 11° 22′, in 23 years and 14 days.

It does not appear that the distance has undergone any perceptible alteration.

As we have now the proper motion of this star in Dr. MASKELYNE'S new Tables, we are enabled to enter upon an examination of the cause of the observed change; but first it will be necessary to mention, that in this and all the following stars, I have no longer supposed the solar motion to be directed towards λ Herculis. A point at no very great distance from this star has been chosen, for reasons which it would lead us too far from our present subject to assign, and which are of no absolute consequence to it. The motion of the solar system, towards this assumed point, will produce an opposite parallactic motion, in every star that is not too far from us to be sensibly affected by it.

That change of place which astronomers have established by observation, and which is called the proper motion of a star, either may agree with this parallactic motion, (in which case it will be only an apparent one, the star being really at rest,) or it may be directed to another part of the heavens, so as to differ from our parallactic motion. Whenever this happens, the star will have the following three motions: a real, a parallactic, and an apparent one; the latter being a composition of the former two.

That α Herculis is one of those stars which has these three motions, will appear thus: the parallactic motion which this star, from its magnitude and consequent proximity, must be

allowed to have, will carry it, in an angle of about $58\frac{1}{2}$ degrees, towards the south-preceding part of the heavens; but the motion assigned to it in the new Tables, has a direction towards the north. Hence it follows, that α Herculis has also a real motion, which, by its composition with the parallactic one, produces the tabular apparent one.

We are now to examine the effect of these three motions, on the position of the two stars of our double star, in order to see how far they will account for the observed change. The two stars are sufficiently different in magnitude, for us to expect a difference of parallax, on a supposition that their distances from us are inversely as their apparent magnitudes. The change of the angle of position, arising from a superior parallactic motion of the large star, would have occasioned a retrograde motion of the small one; but this, by my observations, has moved according to the order of signs; its change of situation, therefore, will admit of no explanation from the effect of parallax.

The real motion of α Herculis, being such as, with the union of the parallactic one, will produce an apparent motion towards the north, is determined by the velocities and directions of the other two motions. It must however be towards the north-following part of the heavens, and of a velocity considerably greater than the proper motion given in the new Tables; but, since it is known to be compounded with the parallactic one, we are now only to consult the direction and velocity of that composition, which is such that the large star, in 23 years and 14 days, must have been carried 5″,299 towards the north. If the stars are not connected, the most favourable case we can put, will be to suppose the small one at rest, and at such a distance from us as to be intirely free from sensible parallax.

This being admitted, the large star, by its motion, should now have left the small one so far behind, that the distance of the centres of the two stars, (which Sept. 25, 1781, by a measure with my lamp micrometer, was 4″ 34‴,) should now be 7″,92; while, at the same time, the angle of position ought to have increased to $52\frac{1}{2}$ degrees. My last observations, however, give so different a result, that this hypothesis cannot be admitted.

If the small star, which is not so much less than the large one that we can justly place it at the above mentioned distance, should partake of some parallactic motion, it will then increase the objections we have stated; for, if the effect of it should be only one quarter of what it is upon the large star, it will add to the magnitude of the angle of position, and increase the distance of the two stars.

Hence it follows, that, unless we should admit the supposition of three independent motions, the high improbability of which has been sufficiently shown, we have good reason to believe that the large star has, during the 23 last years, carried the small one along with itself, in the path it describes in space; both being equally affected by parallax and real motion. If this be admitted, a mutual revolution of the two stars will be the immediate consequence, when the laws of gravitation are taken into consideration; and the change of position they have undergone, will be a necessary consequence of it.

γ Arietis. III, 9.

This star being only of the 4th magnitude, and of the third class as a double star, we have no reason to expect a great change in the angle of position; and yet, with the assistance of a very distant observation, which we have in MAYER'S Zodiacal Catalogue, a considerable change may be proved to have taken place.

The position, Nov. 2, 1779, was 84° 0′ south-following.* Oct. 10, 1780, it was 86° 5′; and, Feb. 7, 1802, it was 89° 10′. The change, in 22 years and 97 days, is 5° 10′. From the given right ascension and declination of the two stars, in MAYER's Catalogue, we compute, that their position in 1756 was 78° 46′ south-following; which gives a change of 5° 14′, in 23 years and 306 days, up to the time of my first observation. The two periods, which are nearly equal, give a change of 10° 24′, for 46 years and 38 days. A motion of γ Arietis, arising from systematical parallax, by which we may admit the smallest of the two stars (on account of its supposed greater distance) not to be so much affected as the large one, will perfectly account for the change; unless, hereafter, the proper motion of this star, when known, should lead to a different conclusion.

ξ *Ursæ.* I, 2.

This double star has undergone a very extraordinary change in the angle of position. Dec. 19, 1781, the smallest of the two stars was 53° 47′ south-following. Feb. 4, 1802, it was 7° 31′; and, January 29, 1804, the position was only 2° 38′. This gives a motion of 51° 9′, for 22 years 41 days, and amounts to 2° 19′ *per* year. If an annual alteration to this amount should continue to take place for the future, a very few years would be sufficient to ascertain the cause of this change, as no motion but a revolving one could possibly explain the phenomenon. If, on the contrary, the parallactic motion of the largest star should have occasioned the change of situation, which is not impossible, it will soon be verified by an increased distance of the two stars,

* This position, for reasons explained in the note to *p* Serpentarii, page 374, has not been given in my Catalogue.

accompanied with very little angular change in their position. The little difference in the magnitude of the two stars, however, does not well agree with a supposition which gives a parallactic motion to one of them only.

γ Andromedæ. III, 5.

It has already been noticed, on a former occasion, that this double star is one of the most beautiful objects in the heavens. The striking difference in the colour of the two stars, suggests the idea of a sun and its planet, to which the contrast of their unequal size contributes not a little. The position of the small star, when we consider that this double star is one of the third class, has undergone a sufficient change to deserve notice. In the year 1781, Oct. 15, it was 19° 37′ north-following. Feb. 3, 1802, 26° 34′. Feb. 11, 1803, 26° 5′; and, Feb. 5, 1804, 27° 39′. The difference, in 22 years and 113 days, is 8° 2′. The distance of the two stars is too great to be accurately estimated by their apparent diameters; and measures taken with a micrometer, unless fractions of a second of space could be strictly ascertained, would be useless. If we suppose the small star sufficiently removed not to partake of the systematical parallax of the large one, the change of the angle of position may be accounted for, upon the principle of the solar motion. The stars, however, are hardly so different in magnitude as would be required for that purpose. We ought also to know, whether a proper motion has been observed in this star.

μ Draconis. II, 13.

The change in the relative situation of the two stars of this double star is pretty considerable. The position, Sept. 24, 1801,

was 37° 38′. This may stand either for south-preceding or north-following, because the stars were then regarded as being equal. March 4, 1802, a measure of the position gave 50° 32′. Feb. 5, 1804, position 49° 0′ south-preceding; and, Feb. 6, 1804, 50° 4′. A memorandum annexed to the observation says, that the preceding star is the smallest, but that the difference is so little as to require much attention to be perceived. The alteration, in 22 years and 135 days, is 12° 26′. The two stars being nearly of an equal magnitude, we can have no inducement to suppose them to be at very different distances from us. This makes it not probable that the difference of their parallactic motion should be the cause of the angle of position; otherwise, the direction of that motion would be sufficiently favourable.

δ Geminorum. II, 27.

The measures of the position of the two stars of this double star are attended with great difficulty, on account of the faintness of the smallest; a considerable disagreement will therefore be excuseable. The position, Nov. 18, 1781, was 85° 51′ south-preceding. Jan. 28, 1802, it was 76° 21′. Feb. 4, 1802, 73° 5′; and, Feb. 6, 1804, 69° 52′. The difference, in 22 years and 80 days, is 15° 59′. We can have no assistance from observations made on the distance of the two stars, which is too great for estimation. A parallactic motion, which, on account of the great difference in the magnitude of the stars, might be admitted, would lessen their distance, and make the angle of position retrograde, which, by my observation, has moved in a contrary direction. A connection between the two stars is also rendered improbable, on account of the great number of small ones that are scattered in this neighbourhood, of which our small star may be one;

so that we have good reason to ascribe the change which has happened in the situation of our two stars, to a proper motion of δ.

ε Draconis. I, 8.

In this star, we have to notice a great change of the angle of position, but none in the distance. In the year 1782, Sept. 4, with 460, I found the stars to be $1\frac{1}{2}$ diameter of L. asunder. May 22, 1804, they were still at the same distance of $1\frac{1}{2}$ diameter of L. Oct. 20, 1781, the position was 63° 14′ north-preceding; and, May 22, 1804, it was 84° 29′; which proves a change of 21° 15′, in 22 years and 214 days. This cannot be owing to a parallactic motion of the large star; for the effect arising from such a motion, would have been directly contrary to the change which has taken place: the angle of position would have undergone a direct, instead of a retrograde alteration. We are consequently assured that ε Draconis cannot be at rest. If future observations on the proper motion of the stars should furnish us with that of ε, and if this motion should also fail to explain my observed change of the angle of position, without a change of distance, we shall then have good reason to admit this star into the list of those that have a small one revolving about it. For, to ascribe an additional and independent motion to the small star, would be to have recourse to three separate motions, of given velocities, in given directions, and at given distances; the improbability of which has been sufficiently pointed out.

ζ Aquarii. II, 7.

The position, Nov. 26, 1779, was 71° 5′ north-following. Sept. 24, 1781, it was 71° 39′. June 19, 1782, 72° 7′. Jan. 3, 1802. 78° 3′. The change is 6° 58′, in 22 years and 38 days.

As the equality of the two stars gives little room for admitting a difference in their parallactic motions, we cannot reasonably ascribe the change of situation to that cause; though, otherwise, the direction of such a motion in the largest of the two, would be sufficiently favourable. The situation of the stars being much insulated, a connection between them may be admitted, with a high degree of probability.*

ξ Bootis. II, 18.

The change in the situation of the two stars of this double star is very remarkable. The small star, April 15, 1782, was 65° 53′ north-following the large one. In one of my *sweeps*, April 20, 1792, I perceived the small star in the 20-feet reflector; and estimated its position, as it passed the field of view, to be about 85° north-preceding. When the sweep was finished, I found that this star could not be in the situation I had just seen it, unless it had undergone a considerable change since the year 1782; and, that no mistake had been made in the estimation of this evening, appeared very clearly, by a measure taken of its position, which actually gave 85° 43′,5 north-preceding. This pointed out a retrograde motion of the small star. March 22, 1795, the position was 84° 56′. April 1, 1802, 82° 57′; and, April 2, 1804, I found it 83° 54′. A mean of the two last measures, will give the present situation 83° 26′ north-preceding; and the total change of the angle of position, in 21 years and 352 days, will be 30° 41′.

If it should be remarked, that the measure taken in 1795

* The calculation of the probability of a connection, which has been given in the Phil. Trans. for 1802, page 484, makes it above 75 millions to 1, that these two stars are not situated as they are, by a mere casual scattering of them in space.

appears to be inconsistent, it ought to be recollected, that the cause of this apparent motion remains to be investigated. If the largest of the two stars should pass closely by the smallest, which, on account of its supposed great distance from us, may appear fixed, a very great and quick alteration in the angle of position will take place; but, in a short time the change will become very moderate, and not long after insensible. The same appearances may also happen, although the small star should not be fixed, but revolve about the large one; for, if its orbit were in a plane with the line of sight, it would be seen to move with great velocity, about the opposition, and soon after appear to be almost stationary. That either one or the other of the stars has really had a motion approaching to a straight line, is ascertained from an alteration of the distance; for, in the year 1781, the vacancy between the two stars, with 460, was 3 diameters of the large one. But, April 2, 1804, with 527, their distance was greater than estimations by diameters can determine; and, comparing ξ with π Bootis, I found that the stars of ξ were farther asunder than those of π; notwithstanding, in the year 1782, the former was placed in the 2d class, and the latter in the 3d. The change of the angle of position, if it were owing to a parallactic motion, would have been direct, instead of retrograde.

ω *Leonis.* I, 26.

In a note added to this star, which is the 26th in my second Catalogue, a suspicion is expressed, that the two stars which compose this very minute double star, were receding from each other.* This has since been completely verified; for, having seen the two stars close upon one another, and afterwards by

* See Phil. Trans. Vol. LXXV. Part I. page 48.

degrees disengaged, as related in my second Catalogue, the separation between them kept on increasing, and, on the 21st of April, 1795, they were $\frac{1}{2}$ diameter of the small star asunder. Feb. 5, 1804, with a power of 527, the vacancy between them was nearly 1 diameter of the small one. The position has likewise undergone a sensible alteration. Nov. 13, 1782, it was 20° 54′ south-following. Feb. 4, 1802, 41° 28′. Feb. 5, 1804, 40° 17′. A mean of the two last measures, is 40° 53′ The change, therefore, amounts to 19° 59′, in 21 years and 84 days, and is probably owing to a real motion of ω Leonis; for the effect of a parallactic motion would have shown itself in a contrary alteration of the angle of position.

π *Arietis.* I, 64.

This star is marked as being treble; and the third star, as it happens, is now of use, in verifying the measures which have ascertained the relative change in the situation of the other two. The position of π and its adjacent star, Oct. 29, 1782, was 19° 9′ south-following; and the third star was in the same line of that angle continued. Oct. 17, 1802, the position was 34° 11′; and, Feb. 6, 1804, by a mean of two measures, 31° 15′; which gives a change of 12° 6′, in 21 years and 100 days.

That this change has taken place gradually, is confirmed by two observations of the third star. Jan. 15, 1795, the distant star was observed to have remained a little behind, while the near one had advanced; and, Oct. 17, 1802, it was again remarked, that the three stars were no longer in a line, and that the nearest small star had advanced according to the order of the signs, which had increased its angle of position.

The multitude of small stars in this neighbourhood, and the

minuteness of the two that have been observed with π, as well as the distance of the farthest, render a connection between the three stars very improbable; nor can the change of situation be owing to parallax, as this would have occasioned a retrograde motion of the small star, which, on the contrary, has been direct. From these considerations we may conclude, that π Arietis has a proper motion, to which we must look for the cause of the observed change.

η *Coronæ*. I, 16.

This very minute double star has undergone a great alteration in the relative situation of the two stars. Sept. 9, 1781, their position was 59° 19′ north-following; and, Sept. 6, 1802, by a mean of two very accurate measures, it was 89° 40′ north-preceding; which amounts to a change of 31° 1′, in 20 years and 362 days. The distance of the two stars has not been subject to any sensible alteration. Sept. 9, 1781, a very small division might be seen, with 460. August 30, 1794, they were so close that, with a 10-feet reflector, and power of 600, a very minute division could but just be perceived. April 15, 1803, with a 10-feet reflector, a very small division was also visible, with 400, though better with 600. And, May 15, 1803, I saw the separation between the two stars, with the same 7-feet reflector, and magnifying power of 460, with which I had seen it 22 years before. The stars differ very little in magnitude; so that we have no reason to expect any effect from a difference of parallax. Besides, if the small one were out of the reach of it, a parallactic motion of the largest alone, would have occasioned the small one to move apparently according to the order of the signs; but the motion has been retrograde.

Fl. 21 *Ursæ.* II, 73.

Nov. 17, 1782, the two stars were in the position of 36° 45′ north-preceding; and, May 20, 1802, I found them 47° 37′; which gives a change of 10° 52′, in 19 years and 184 days. A parallactic motion will account for it; unless, hereafter, a proper motion of the large star should be found to have a different tendency.

Fl. 4 *Aquarii,* I. 44.

The position of the two stars, July 23, 1783, was 81° 30′ north-preceding; and, by a mean of two observations, August 28 and 29, 1802, it was 61° 5′ north-following. Both the last measures are positive, with regard to the position being following, and not preceding, as it certainly was in the year 1783. This proves a change of 37° 25′, in 19 years and 37 days. The distance is perhaps a little increased. Sept. 5, 1782, it was $\frac{1}{6}$ diameter of S. August 29, 1802, less than $\frac{1}{2}$ diameter of S. A parallactic motion of the large star, would have brought on a retrograde motion of the small one, which, on the contrary, we find has been direct. This proves a real motion, the nature of which cannot remain many years unknown; its velocity, hitherto, having been at the rate of nearly 2 degrees *per* year, of angular change.

South-preceding π Serpentis. I, 81.*

The position, March 7, 1783, was 49° 48′ south-preceding. August 30, 1802, it was 59° 5′. The change is 9° 17′, in 19 years and 176 days. If the stars were a little more different in

* We now have the place of this double star in Bode's Catalogue, where it is called 112 Serpentis.

magnitude, a parallactic motion of the largest would account for the change of position.

Near μ Bootis. I, 17.

There is a considerable change in the relative situation of the two stars of this double star; and, by the assistance of μ Bootis, it is remarkably well ascertained. This star is so near, that it may be brought into the same field of view with our double star. Sept. 3, 1782, the position was 87° 14′ north-preceding; and, about a year before, the situation of μ Bootis had been determined, so that it appeared, from the two measures, that the three stars were almost in a line, the small star being, however, 6° 49′ on the *following* side. August 30, 1802, the position of the small star was 76° 14′ north-preceding; which, in 19 years and 361 days, gives a change of 11° 0′; and it was at the same time observed, that when all the three stars were seen together, the small one was on the *preceding* side of the line which joins this double star and μ Bootis. A parallactic motion of the large star, would have occasioned the small one to go in a direct order; but it has had a retrograde motion.

North-preceding Fl. 18 *Persei.* I, 38.*

The two stars, August 20, 1782, were situated in a direction 8° 24′ north-preceding; and, by a mean of two measures, taken March 7, 1804, the position was 20° 34′. This gives a change amounting to 12° 10′, in 21 years and 199 days. There is probably a little increase in the distance of the stars. The first observations, with 460, give $\frac{1}{2}$ diameter of either of them,

* The place of this star is now given in BODE's Catalogue, where it is the 85th Persei.

supposing the stars to be equal; and the last, with 527, make it a diameter of the smallest; the stars being then considered as pretty unequal. If the difference of the parallactic motion of the two stars should be sufficiently considerable, that motion would account, not only for the change of the angle of position, but also for a small increase of the distance of the two stars.

σ Coronæ. I, 3.

This star has undergone a great change. The position of the two stars, Oct. 15, 1781, was 77° 32′ north-preceding; but, Sept. 6, 1802, it was 78° 36′ north-following; which gives an alteration of 23° 52′, in 20 years and 326 days. The great number of small stars in this neighbourhood, is not favourable to a supposed connection between any of them and σ Coronæ. As the two stars are considerably unequal, we may suppose the large one to be affected by a parallactic motion, which will sufficiently account for the angular change.

ε Lyræ. II, 5 and 6.

This remarkable double-double star has undergone a change of situation in each double star separately, which is not very considerable, but deserves our notice, on account of a certain similarity in the directions of the alteration. The position of II, 5, Nov. 2, 1779, was 56° 5′ north-following; and, by a mean of three observations, taken Sept. 20, 1802, May 26, and 29, 1804, it was 59° 14′; which gives a change of 3° 9′; the motion of the angle being retrograde. The position of II, 6, on the same days, was 83° 28′, and 75° 35′, south-following. This gives a difference of 7° 53′; the motion being also retrograde. Now, from the position of the apex of the translation of the solar

system, it follows, that the parallax arising from this principle, cannot account for the motion of both the sets of double stars: it may explain the change of the preceding, but not of the following one. The situation of both, however, is in a part of the heavens which is so rich in scattered small stars, that a variety of casual, and merely apparent combinations may be expected.

p Serpentarii Fl. 70. II, 4.

The alteration of the angle of position, that has taken place in the situation of this double star, is very remarkable. Oct. 7, 1779, the stars were exactly in the parallel, the preceding star being the largest; the position therefore was 0° 0′ following.* Sept. 24, 1781, it was 9° 14′ north-following; and, May 29, 1804, it was 48° 1′ north-preceding; which gives a change of 131° 59′, in 24 years and 234 days. This cannot be owing to the effect of systematical parallax, which could never bring the small star to the preceding side of the large one.

λ *Ophiuchi*. I, 83.

The position, March 9, 1783, was 14° 30′ north-following. May 20, 1802, it was 20° 41′. The difference, in 19 years and 72 days, is 6° 11′. March 9, 1783, the distance, with 460, was $\frac{1}{4}$ or $\frac{1}{3}$ diameter of the small star. May 1 and 2, 1802, I could not perceive the small star, though the last of the two evenings was very fine. May 20, 1802, with 527, I saw it very well, but

* The first position was not given in my Catalogue, as I had no reason to suppose, at the time of its publication, that the positions of the stars were liable to any progressive change. It may be remembered, that my principal aim was, if possible, to find out some small annual variation, or libration of position, which might lead to a discovery of the parallax of the fixed stars.

with great difficulty. The object is uncommonly beautiful; but it requires a most excellent telescope to see it well, and the focus ought to be adjusted upon ε of the same constellation, so as to make that perfectly round. The appearance of the two stars is much like that of a planet with a large satellite or small companion, and strongly suggests the idea of a connection between the two bodies, especially as they are much insulated. The change of the angle of position, might be explained by a parallactic motion of the large star; but the observations on the distance of the two stars, can hardly agree with an increase of it, which would have been the consequence of that motion.

North-preceding Fl. 29 *Capricorni.* I, 47.

The position, July 23, 1783, was 84° 48′ north-preceding. Sept. 1, 1802, it was 66° 50′. This gives a change of 17° 58′, in 19 years and 40 days. The effect of a parallactic motion would fall chiefly on the distance; it will, however, account for the change of the angle.

Near Fl. 3 *Pegasi.* II, 62.

The position, May 3, 1783, was 88° 24′ north-preceding. August 31, 1802, it was 79° 38′ south-following. The change is 8° 46′, in 19 years and 120 days. The stars are so nearly equal, that in 1783 I supposed the preceding one to be the smallest, and in 1802 the following one; which occasions the different denomination of the angles of position. If the distance of the preceding star should be much greater than that of the following one, a parallactic motion would explain the change of the angle, but not otherwise.

Fl. 49 *Serpentis.* I, 82.

In the year 1783, March 7, the position of the two stars of this double star, was 21° 33′ north-preceding. May 20, 1802, 32° 52′; and, April 2, 1804, 35° 10′; which gives a change of 13° 37′, in 21 years and 26 days. The stars are now a little farther asunder than they were formerly. A parallactic motion would account for the change of the angle, but not for the increased distance.

Preceding Fl. 11 *Serpentarii.* II, 23.

The position of the stars, May 18, 1782, was 46° 24′ north-preceding. May 20, 1802, it was 66° 56′; which gives a change of 20° 32′, in 20 years and 2 days. A parallactic motion, if the small star should be sufficiently distant from us, will account for it.

Fl. 38 *Piscium.* II, 50.

The position, June 30, 1783, was 25° 3′ south-preceding, and, August 31, 1802, it was 34° 43′. The change is 9° 40′, in 19 years and 62 days. The small star has been retrograde. If the change had been owing to the systematical parallax, the motion would have been direct.

Near Fl. 64 *Aquarii.* III, 69.*

The position, August 21, 1783, was 20° 3′ north-following. Oct. 16, 1802, it was 31° 34′. The change, in 19 years and 56 days, is 11° 31′; and may be accounted for by a parallactic motion of the large star, especially as the stars are extremely unequal in apparent magnitude.

* In BODE's Catalogue, it is now called 222 Aquarii.

Fl. 46 *Herculis.* I, 79.

There is a small change in the distance of the two stars of this double star. Feb. 5, 1783, the interval between them, with 227,* was nearly 1 diameter of L, and with 460, $1\frac{3}{4}$ diameter of L. Sept. 29, 1802, it was $2\frac{1}{2}$ or 3 diameters of L. The position, Feb. 5, 1783, was 66° 36′ south-following. Sept. 29, 1802, it was 76° 18′. The alteration is 9° 42′, in 19 years and 236 days; but cannot be owing to parallactic motion.

δ *Cygni.* I, 94.

This double star, I believe, has furnished us with a second instance of a conjunction, resembling that of ζ Herculis. The position, Sept. 22, 1783, was 18° 21′ north-following. Jan. 3, 10, and 11, 1802, I could no longer perceive the small star; which must have been at least so near the large one as to be lost in its brightness. Jan. 29, 1804, I examined this star with powers from 527 to 1500, and saw it as a lengthened star, but not with sufficient clearness to take a measure of its position. May 22, 1804, in a very clear evening, I tried 527 and 1500, with the 10-feet reflector, which acted remarkably well on other double stars, but I could not perceive the small star of δ Cygni. In hopes that the superior light of a 20-feet reflector would show it, I examined the star, May 29, 1804, with the powers 157 and 360, but could not perceive the small one. A parallactic motion of δ will perfectly account for this occultation; for the situation of the two stars, in 1783, was such, that this motion

* In my Catalogue, the power is called 460, instead of 227, as it should have been; and the rest of the observation, with 460, was by mistake omitted.

must have carried the large star, by this time, nearly upon the small one.

b Draconis. I, 7.

The position, Oct. 10, 1780, was 77° 19′ north-following; and, Oct. 30, 1802, it was 83° 41′. The change is 6° 22′, in 22 years and 20 days. The effect of a parallactic motion of the largest star, would have shown itself in a direction contrary to the observed one; a proper motion of one of the stars, at least, must be admitted.

South-preceding Fl. 30 *Orionis.* I, 75.

The position, Jan. 9, 1783, was 89° 36′ north-preceding; and, Jan. 22, 1802, it was 79° 12′ north-following; which gives a change of 11° 12′, in 19 years and 13 days. A parallactic motion of either of the stars, for they are nearly equal, would chiefly affect their distance; besides, the stars are so numerous in this part of the heavens, that we can only look upon this as a casual double star; a proper motion therefore must be recurred to.

η Cassiopeæ. III, 3.

The situation of the two stars of this beautiful double star, June 14, 1782, was 27° 56′ north-following; and, Feb. 11, 1803, it was 19° 14′; which gives a change of 8° 42′, in 20 years and 242 days. This arises probably from a real motion of η in space; for parallax would have had a contrary effect.

d Serpentis. I, 12.

This star has not altered its angle of position sufficiently to be certain of the change, which only amounts to 2° 8′; this

quantity being too small for the precision of the micrometer, when only two measures are taken; but the alteration in the distance of the two stars is well ascertained. Oct. 22, 1781, with 278, it was $1\frac{1}{3}$ diameter of L. April 28, 1783, with 460, it was $2\frac{1}{2}$ diameters; and, May 4, 1802, it was not less than 4 or 5 diameters of L. If this change had arisen from a parallactic motion, there must have been a considerable alteration in the angle of position, which cannot be admitted; it may, therefore, more properly be ascribed to a real motion of *d* Serpentis.

North of 105 *Herculis.* I, 86.

The alteration in the angle of position of this star is uncommonly great. April 27, 1783, it was 79° 24′ north-preceding; and, Sept. 29, 1802, it measured only 22° 27′; which denotes a change of 56° 57′, in 19 years and 155 days. The distance has undergone very little alteration, but is rather less now than it was formerly. A real motion of the largest star, in a north-following direction, may explain this change, which cannot be ascribed to a parallactic motion of the stars.

Rigel. II, 33.

This bright star has undergone a change of situation with regard to its distance from the small one, which is near it; but, in the angle of position, very little difference can be perceived. By eleven measures, taken between Jan. 1, 1802, and Feb. 18, 1803, the mean position is 69° 5′ south-preceding; which is but little more than 68° 12′, the measure of Oct. 1, 1781, given in my Catalogue.

The distance was estimated, Oct. 1, 1781, with 460, to be more than 3 diameters of Rigel; and, as I supposed it to be

one of those double stars of which I might ascertain the vacancy between the two stars, by estimating the number of diameters of the large one that would fill it up, I placed the star in the second class. However, by a measure taken with a micrometer, Oct. 22, 1781, the stars were found to be far enough asunder to come into the third class. By a mean of six measures, which were taken the first 18 months of my observing the star, their distance was 9″ 32‴; and, by a repetition of estimations, it appeared, Dec. 22, 1781, that the vacancy between the two stars was not less than 4 diameters, and, when the air was tremulous, 4 or 5. After an interval of more than 21 years, having omitted estimations by the diameter, as not very proper to be used with these stars, I wished to compare their distance with the former estimations; and, with the same instrument and same magnifying power that had been used before, the vacancy, Feb. 22, 1803, amounted to 5 or 6 diameters of the large star; so that, certainly, an increase of distance must be admitted.

The number of scattered stars in this neighbourhood, and the smallness of the star to which the relative situation of Rigel has been referred, render it probable that there is only a casual proximity, and no real connection, between these two stars. Nor can the change of their relative situation be accounted for by a parallactic motion of Rigel, although we should admit the small star to be without the reach of solar parallax; for the effect arising from parallactic motion, would not only lessen the distance of the two stars, but would occasion a considerable diminution in the angle of position, neither of which have taken place.

As we have now the proper motion of Rigel, in Dr. MASKELYNE'S new Tables, we can no longer be at a loss for the cause of the change; for, by a composition of the tabular motions in

right ascension and polar distance, this star, in 21 years and 144 days, must have moved about 3″,481, in an angle of 79° 29′ 33″, towards the north-following part of the heavens. This would consequently remove it from the small star, which is placed almost in an opposite direction, and would occasion hardly any change in its angle of position; and these are the very phenomena which have been established by my observations.

ζ Cancri. III, 19.

The position of the stars, Nov. 21, 1781, was 88° 16′ south-preceding; and, Feb. 7, 1802, it was 81° 47′ south-following. The change is 9° 57′, in 20 years and 78 days; and may be ascribed to a parallactic motion of the large star, which is in favour of the observed alteration.

ρ Capricorni. II, 51.

The position, July 4, 1783, was 84° 0′ south-following; and, August 29, 1802, it was 86° 55′ south-preceding. This gives a change of 9° 5′, in 19 years and 56 days; and a motion arising from parallax will sufficiently account for it.

North-preceding Fl. 56 *Andromedæ.* I, 89.*

The position, July 28, 1783, was 75° 30′ south-following; and, Sept. 19, 1802, it was 67° 4′. The change is 8° 26′, in 19 years and 53 days. A parallactic motion of the large star would have occasioned the change of the angle to be direct, instead of retrograde.

* The 241st Andromedæ of Bode's Catalogue, gives us now the place of this star.

Near 37 Aquilæ. I, 13.*

The position, Oct. 6, 1782, by a mean of two measures, was 37° 15′ north-preceding; and, Oct. 2, 1802, it was 44° 45′. The change is 7° 30′, in 19 years and 361 days; and may be owing to a parallactic motion.

α Ursæ minoris. IV, 1.

There has been a small alteration in the relative situation of the pole star; but, when we consider that this double star is of the fourth class, we cannot expect that any great change in the angle of position should have taken place, in the course of 20 years. The position, Dec. 19, 1781, was 66° 42′ south-preceding; and, June 17, 1782, it was 67° 23′. A mean of both measures, is 67° 3′. March 4, 1802, the position was 61° 43′; which gives a difference of 5° 20′, in 19 years and 350 days. A parallactic motion of the large star, which, considering the great difference of size between the two, may well be admitted, will account for the angular change; especially as the distance of the two stars exceeds the limits which probability points out for connected stars, when the large one is of the third magnitude.

North-preceding Fl. 62 *Aquilæ.* I, 93.

The position, Sept. 12, 1783, was 19° 9′ north-preceding; and, Oct. 2, 1802, it was 13° 21′. The change is 5° 48′, in 19 years and 20 days. A parallactic motion of the largest of the two stars, would have occasioned a contrary apparent motion of the small one.

* The place of this star is now given in BODE's Catalogue, where it is 136 Aquilæ.

Preceding τ Orionis. I, 54.

The position, January 22, 1783, was 35° 42′ north-preceding; and, Jan. 25, 1802, it was 41° 27′. The change is 5° 45′, in 19 years and 3 days; and may be owing to the effect of parallax.

ζ Ursæ majoris. III, 2.

The position, Nov. 18, 1781, was 56° 46′ south-following; and, Oct. 3, 1802, it was 51° 14′. The change is 5° 32′, in 20 years and 319 days; but this cannot be accounted for by a parallactic motion of ζ, which would have occasioned a contrary change of the angle.

North-following φ Herculis. I, 37.

The position, Oct. 6, 1782, was 59° 48′ south-following; and, Sept. 20, 1802, it was 65° 0′; which gives a change of 5° 12′, in 19 years and 349 days. It cannot be ascribed to a parallactic motion of the largest star.

North-following ν Aquarii. I, 46.

The position, July 31, 1783, was 62° 27′ north-preceding; and, August 29, 1802, it was 67° 27′. The change is 5° 0′, in 19 years and 29 days. The distance of these stars is now greater than it was formerly. July 31, 1783, with 460, they were rather more than 1 diameter asunder. August 29, 1802, I found them too far distant to be put into the first class. If any effect of parallax can reach such small stars, it is so far in favour, that it will account for an increase of the distance, but not for the change of the angle of position.

α Piscium. II, 12.

The position of the stars, Oct. 19, 1781, was 67° 23′ north-preceding; and, by a mean of three measures, taken Jan. 28 and Feb. 4, 1802, it was 63° 0′. This gives a change of 4° 23′, in 20 years and 105 days. The parallactic motion of *α* will account for the alteration, unless a proper motion should hereafter lead to a different conclusion, which, from the insulated situation of this double star, is not improbable.

Fl. 11 *Monocerotis.* II, 17.

The position, Oct. 20, 1781, was 31° 38′ south-following; and, by a mean of two measures, taken Feb. 4, and March 4, 1802, it was 27° 12′. The change, which is 4° 26′, in 20 years and 121 days, may be accounted for by a parallactic motion.

North-preceding γ Aquilæ. I, 91.

The position, August 7, 1783, was 8° 18′ north-preceding; and, Sept. 20, 1802, it was 12° 23′. This gives a change of 4° 5′, in 19 years and 44 days; and may be accounted for upon the principles of parallax.

e Geminorum. III, 47.

The position, Oct. 2, 1782, was 89° 54′ south-following; and, April 6, 1802, it was 86° 6′ south-preceding; which gives a change of 4° 0′, in 19 years and 186 days. This cannot be ascribed to parallactic motion.

Fl. 32 *Eridani.* II, 36.

The position, Oct. 22, 1781, was 73° 23′ north-preceding; and, Feb. 6, 1804, it was 77° 19′. The change is 3° 56′, in 22 years and 107 days. It cannot be owing to a parallactic motion, which would have produced a different effect.

Fig. 1.

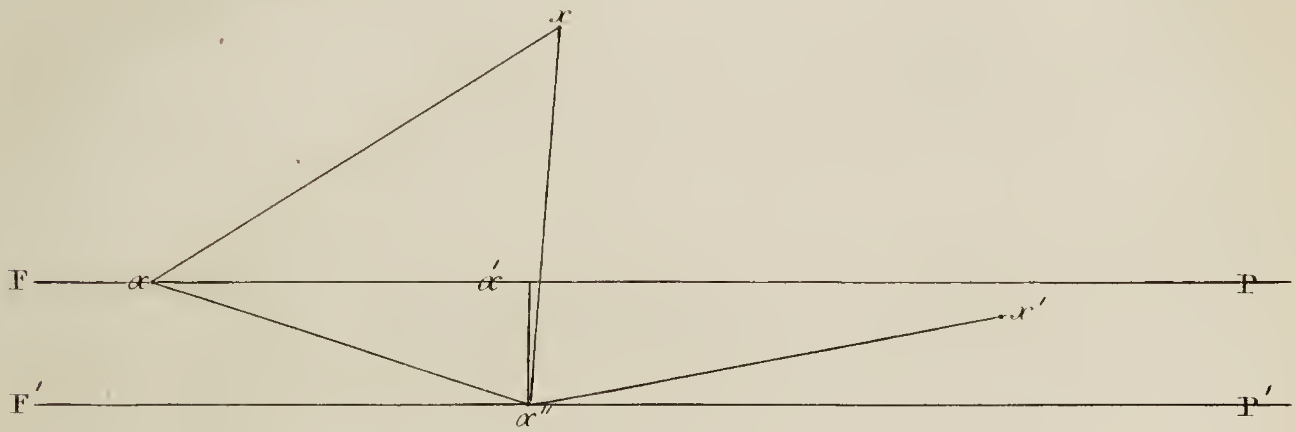
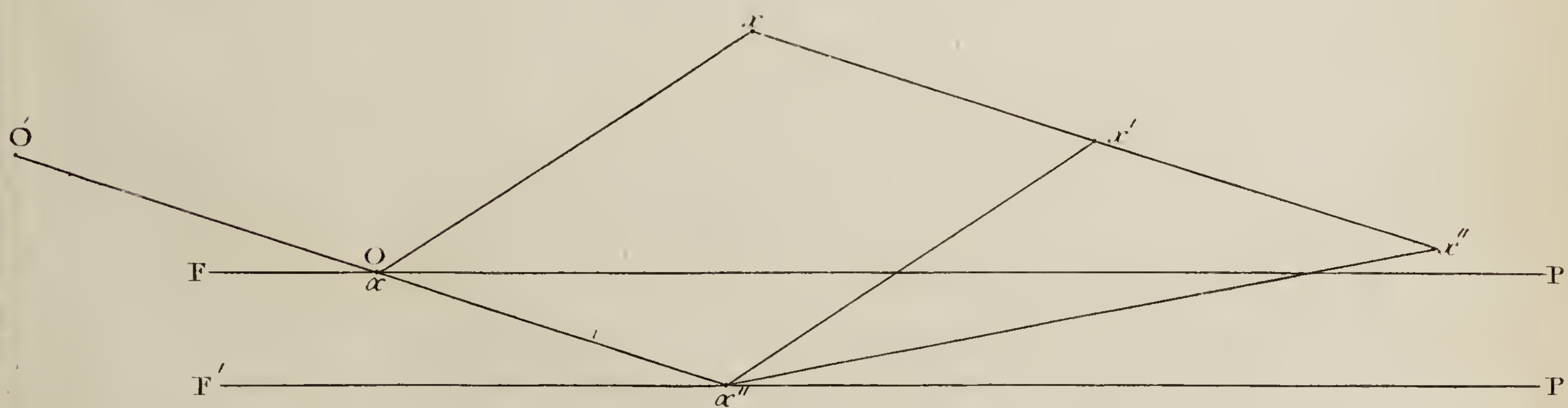


Fig. 2.



Basire sc.

XV. *Observations on the Change of some of the proximate Principles of Vegetables into Bitumen; with analytical Experiments on a peculiar Substance which is found with the Bovey Coal. By* Charles Hatchett, *Esq. F. R. S.*

Read June 14, 1804.

§ I.

One of the most instructive and important parts of geology, is the study of the spontaneous alterations by which bodies formerly appertaining to the organized kingdoms of nature have, after the loss of the vital principle, become gradually converted into fossil substances.

In some cases, this conversion has been so complete, as to destroy all traces of previous organic arrangement; but, in others, the original texture and form have been more or less preserved, although the substances retaining this texture, and exhibiting these forms, are often decidedly of a mineral nature. Some, however, of these extraneous fossils (as they are called) retain part of their original substance or principles, whilst others can only be regarded as casts or impressions.

From the animal kingdom we may select, as examples, the fossil ivory, which retains its cartilage;* the bones in the Gibraltar rock, consisting of little more than the earthy part or

* I have also found the cartilage perfect, in the teeth of the mammoth.

phosphate of lime; the shells forming the lumachella of Bleyberg, which still possess the lustre and iridescence of their original nacre; and the shells found at Hordwell in Hampshire, and in Picardy, which are chiefly porcellaneous, but more or less calcined; also the fossil echini and others, so commonly found in the limestone, chalk, and calcareous grit of this island, which, although they retain their original figure, are intirely, or at least externally, formed of calcareous spar, incrusting a nucleus of flint or chalcedony. And if, in addition to these, we may be allowed to regard the more recent limestone and chalk strata as having been principally or partly formed from the detritus of animal exuviæ, we shall possess a complete series of gradations, commencing with animal substances analogous in properties to those which are recent, and terminating in bodies decidedly mineral, in which all vestiges of organization have been completely destroyed.

The vegetable kingdom has likewise produced many instances not less remarkable; and it is worthy of notice, that animal petrifactions are commonly of a calcareous nature, while, on the contrary, the vegetable petrifactions are generally siliceous;*

It is not, however, my intention here to enter into a minute discussion concerning the formation of these extraneous fossils; I shall therefore proceed to consider other equally or perhaps more important changes, which organized bodies, especially vegetables, appear to have suffered, (after the extinction of the principle of life,) by being long buried in earthy strata, and by being thus exposed to the effects of mineral agents.

* Pyrites, ochraceous iron ore, and fahlertz, are also occasionally found in the forms of vegetable bodies.

§ II.

The principal object I have in view, is to adduce some additional proofs, that the bituminous substances are derived from the organized kingdoms of nature, and especially from vegetable bodies; for, although many circumstances seem to lead to the opinion, that the animal kingdom has in some measure contributed to the partial formation of bitumen, yet the proofs are by no means so numerous, nor so positive, as those which indicate the vegetable kingdom to have been the grand source from which the bitumens have been derived. But this opinion, (founded upon very strong presumptive evidence,) although generally adopted, is however questioned by some persons; and I shall therefore bring forward a few additional facts, which will, I flatter myself, contribute to demonstrate, that bitumen has been, and is actually and immediately formed, from the resin, and perhaps from some of the other juices of vegetables.

The chemical characters of the pure or unmixed bitumens, such as naptha, petroleum, mineral tar, and asphaltum, are, in certain respects, so different from those of the resins and other inspissated juices of recent vegetables, that, had the former never occurred but in a separate and unmixed state, no positive inference could have been drawn from their properties, in proof of their vegetable origin. Fortunately, however, they have been more frequently found under circumstances which have strongly indicated the source from whence they have been derived; and much information has been acquired from observations made

on the varieties of turf, bituminous wood, and pit coal, on the nature of their surrounding strata, on the vestiges of animal and vegetable bodies which accompany them, and on various other local facts; all of which tend considerably to elucidate the history of their formation, and to throw light upon this interesting part of geology.

Some instances have already been mentioned, which show that fossil animal substances form a series, commencing with such as are scarcely different from those which are recent, and terminating in productions which have totally lost all traces of organization.

Similar instances are afforded by the vegetable kingdom; but, without entering into a minute detail of every gradation, I shall only cite three examples in this island, namely,

1. The submarine forest at Sutton, on the coast of Lincolnshire, the timber of which has not suffered any very apparent change in its vegetable characters.*

2. The strata of bituminous wood (called Bovey Coal) found at Bovey, in Devon; which exhibits a series of gradations, from the most perfect ligneous texture, to a substance nearly approaching the characters of pit coal, and, on that account, distinguished by the name of Stone Coal.

3. And lastly, the varieties of pit coal, so abundant in many parts of this country, in which almost every appearance of vegetable origin has been destroyed.

The three examples abovementioned, appear to form the extremities and centre of the series; but as, from some local

* Account of a submarine Forest on the East Coast of England, by Dr. Correa de Serra. Phil. Trans. for 1799, p. 145.

circumstances, the process of carbonization, and formation of bitumen, has not taken place in the first instance, and as these effects have proceeded to the ultimate degree in the last, it seems most proper that we should seek for information, and for positive evidence, in the second example, which appears to be the mean point, exhibiting effects of natural operations, by which bitumen and coal have been imperfectly and partially formed, without the absolute obliteration of the original vegetable characters; and, although I have selected the Bovey coal as an example, because it is found in this country, we must recollect that similar substances, or strata of bituminous wood, are found in many parts of our globe; so that the example which has been more immediately chosen, is neither rare nor partial.*

The nature, however, of the various kinds of bituminous wood, may in some respects be different; but this I have not as yet had the means of ascertaining; I shall therefore only state the facts resulting from experiments made on Bovey coal, and more especially on a peculiar bituminous substance with which it is accompanied. But, before I enter into these particulars, it will be proper to mention a very remarkable schistus, with which I was, some months since, favoured by the Right Hon. Sir Joseph Banks.

* Strata of bituminous wood are found in various parts of France, in the vicinity of Cologne, in Hesse, Bohemia, Saxony, Italy, and especially in Iceland, where it is known under the name of Surturbrand.

§ III.

This schistus was found by Sir Joseph, in the course of his tour through Iceland, near Reykum, one of the great spouting hot springs, distant about twenty-four English miles from Hafnifiord; but circumstances did not permit him to ascertain the extent of the stratum.

The singularity of this substance is, that a great part of it consists of leaves, which are evidently those of the alder, interposed between the different lamellæ. I do not mean mere impressions of leaves, such as are frequently found in many of the slates, but the real substance, in an apparently half charred state, retaining distinctly the form of the leaves, and the arrangement of the fibres.

The schistus is light, brittle, of easy exfoliation, in the transverse fracture earthy, and of a pale brown colour; but, when longitudinally divided, the whole surface constantly presents a series of the leaves which have been mentioned, uniformly spread, and commonly of a light gray on the upper surface, and of a dark brown on the other; the fibres on the light gray surface being generally of a blackish-brown, which is also the colour assumed by the schistus when reduced to powder.

The leaves appeared to be in the state of charcoal, by being extremely brittle, by the blackish brown colour, by deflagrating with nitre, by the manner of burning, and by forming carbonic acid. I was, however, soon convinced that the substance of these leaves was not complete charcoal, but might more properly be regarded as vegetable matter in an incipient state of carbonization, which, although possessed of many of the

apparent properties of charcoal, still retained a small portion of some of the other principles of the original vegetable.

My suspicion was excited, partly by the odour produced during combustion, which rather more resembled that of wood than that of charcoal, and partly by the brown solution formed by digesting the powder of the unburned schistus in boiling distilled water; for, by various tests I ascertained, that the substance thus dissolved was not of a mineral nature. In order, however, fully to satisfy myself in this respect, I digested 250 grains of the pulverized schistus with six ounces of water.

The liquor was, as before, of a dark brown colour.

It had but little flavour.

Prussiate of potash, muriate of barytes, and solution of isinglass, did not produce any effect; nitrate of silver formed a very faint cloud; sulphate of iron was slowly precipitated, of a dark brownish colour; and muriate of tin produced a white precipitate.

A portion of the solution, by long exposure to the air, was partially decomposed; and a quantity of a brown substance was deposited, which could not again be dissolved in water.

Another portion was also evaporated to dryness, and afforded a similar brown substance, which was only partially soluble in water; and the residuum, in both of the above cases, was found to be insoluble in alcohol, and in ether.

When burned, it emitted smoke, with the odour of vegetable matter.

250 grains of the schistus, afforded about three grains of the above substance; and, when the properties of the aqueous solution are considered, such as its partial decomposition, and the deposit which it yielded by exposure to air, and by evapora-

tion; the insolubility of this deposit when again digested with water, alcohol, or ether; the smoke and odour which it yielded when burned; and the precipitates formed by the addition of sulphate of iron and muriate of tin to its solution; when these properties, I say, are considered, there seems much reason to conclude, that the substance dissolved by water was vegetable extract, which had apparently suffered some degree of modification, but not sufficient to annul the more prominent characteristic properties of that substance.

The powder of the schistus, which had been employed in the preceding experiment, was afterwards digested in alcohol during two days; and a pale yellow tincture was thus formed, which, by evaporation, left about one grain of a yellow transparent substance, possessing the properties of resin.

It appears, therefore, that a substance very analogous to vegetable extract, and a small portion of resin, remain inherent in the leaves of this remarkable schistus.

As solution of isinglass did not produce any effect, there was reason to conclude, that the aqueous solution above-mentioned did not contain any tannin; but, as the tannin might be combined with the alumina of the schistus, I digested a portion of it in muriatic acid, which, after filtration, was evaporated almost to dryness, leaving, however, the acid in a slight excess. This was diluted with water; and afforded a blue precipitate with prussiate of potash, a yellowish precipitate with ammonia, and a white precipitate with muriate of tin, but not any with solution of isinglass. The tannin which might have been contained in the recent vegetable, appears therefore to have been dissipated or decomposed, with the greater part of the other vegetable principles, excepting the woody fibre reduced to the state of an imperfect

coal, and the small portions of extract and resin which have been mentioned.

Previous to having made the analysis, I had an idea, that this schistus might be a lamellated incrustation, formed by the tufa of the hot springs; but, according to Mr. KLAPROTH'S analysis,* the tufa of Geyser is composed of,

Silica	- -	98
Alumina	- -	1.50
Iron	- -	50
		100.

It is therefore very different from the schistus, the component ingredients of which were ascertained by the following analysis.

ANALYSIS OF THE SCHISTUS FROM ICELAND.†

A. 250 grains, by distillation, yielded water, which, in the latter part of the process, became slightly acid and turbid, = 42.50 grains.

B. The heat was gradually increased, until the bulb of the retort was completely red-hot. During the increase of the heat, a thick brown oily bitumen came over, which weighed 7.50 grains; it was attended with a copious production of hydrogen, carbonated hydrogen, and carbonic acid, the whole of which may be estimated at 23.75 grains.

C. The residuum was black, like charcoal, and weighed 176.25 grains; but, being exposed to a strong red heat in a crucible of platina, it burned with a faint lambent flame, and was at length reduced to a pale brown earthy powder, which weighed 122 grains; so that 54.25 grains were consumed.

* *Beiträge; Zweiter band,* p. 109.

† The remaining specimens are now in the British Museum, and in the collection of the Right Honourable CHARLES GREVILLE.

D. The 122 grains were mixed with 240 of pure potash; and, as some particles of charcoal remained, 50 grains of nitre were added, and the whole was strongly heated, during half an hour, in a silver crucible. The mass was then dissolved in distilled water, and, muriatic acid being added to excess, the liquor was evaporated to dryness, and was again digested with muriatic acid much diluted; a quantity of pure silica then remained, which, after having been exposed to a red heat, weighed 98 grains.

E. The liquor from which the silica had been separated, was evaporated nearly to dryness, and added to boiling lixivium of potash; after the boiling had been continued for about one hour, the liquor was filtrated, and a quantity of oxide of iron was collected, which amounted to 6 grains.

F. Solution of muriate of ammonia was added to the preceding filtrated liquor; and, the whole being then heated, a copious precipitate of alumina was obtained, which, after having been made red-hot, weighed 15 grains.

Carbonate of soda caused the preceding liquor (after the separation of alumina) to become slightly turbid, but not any precipitate could be collected.

By this analysis, 250 grains of the schistus afforded,

		Grains.
Water - - -	A. -	42 50
Thick brown oily bitumen	B. -	7.50
Mixed gas (by computation)	B. -	23.75
Charcoal (by computation)	C. -	54.25
Silica - - -	D. -	98
Oxide of iron - - -	E. -	6
Alumina - - -	F. -	15
		247.

But the water and vegetable matter must be regarded as extraneous; and, if they are deducted, the real composition of the schistus is nearly as follows.

Silica - - - -	82.30
Alumina - - -	12.61
Oxide of iron - - -	5
	99.91.

It evidently, therefore, belongs to the family of argillaceous schistus, although the proportion of silica is more considerable than has been found in those hitherto subjected to chemical analysis.

This schistus has not been noticed by von Troil, nor by any of those who have written concerning Iceland; for the slate which was sent to Professor Bergmann by the former, and which is mentioned by the latter in one of his letters, is there expressly stated to be the common aluminous slate containing impressions.*

§ IV.

From the experiments which have been related, we find that the leaves contained in the Iceland schistus, although they are apparently reduced almost to the state of charcoal, nevertheless retain some part of their original proximate principles, namely, extract and resin. This, of itself, is undoubtedly a remarkable

* Letters on Iceland, by Uno von Troil, p. 355.

Mr. Faujas St. Fond has however described a schistus nearly similar, which is found near Roche-Seauve, in the Vivarais. The stratum extends about two leagues; and the only difference is, that, according to Mr. St. Fond, the schistus at Roche-Seauve is of the nature of marle, or, as he terms it, argillo-calcareous, whereas this of Iceland is undoubtedly argillaceous. From Mr. St. Fond's account, it does not appear that the vegetable leaves contained in the schistus of Roche-Seauve have been chemically examined. *Essai de Geologie*, par B. Faujas St. Fond, Tome I. pp. 128 and 134.

fact; but, if it were unsupported by any other, the only inference would be, that the schistus was most probably of very recent formation, and had been produced under peculiar circumstances.

I was desirous, therefore, to discover some similar cases, which might serve as additional corroborative proofs of the gradual alterations by which vegetable bodies become changed, so as at length to be regarded as forming part of the mineral kingdom; and, from the reasons which have been stated in the commencement of this Paper, as well as from a certain similarity in the external characters of the substance composing the leaves above-mentioned with those of the Bovey coal, I was induced to make this last also a subject of chemical inquiry.

In the Philosophical Transactions for the year 1760,* some remarks on the Bovey coal, and an account of the strata, are stated, in a letter from the Rev. Dr. MILLES to the Earl of MACCLESFIELD. The object, indeed, of the author, was to establish that this and similar substances are not of vegetable, but of mineral origin; and, to prove this, he adduces a great number of cases, most of which, however, in the present state of natural history and of chemistry, must be regarded as proving the contrary; whilst others, mentioned by him, such as the Kimmeridge or Kimendge coal, are nothing more than bituminous slates, and of course are of a very different nature.

Dr. MILLES's account of the varieties of the Bovey coal, and of the state of the pits at that time, appears to be very accurate; and, for the present state, or at least such as it was in 1796, I shall beg leave to refer to a Paper of mine, published in the fourth volume of the Transactions of the Linnean Society;†

* Vol. LI. p. 534.

† Observations on bituminous Substances, p. 138. See also PARKINSON's Organic Remains of a former World, Vol. I. p. 126.

for, as this is more immediately a chemical investigation, I wish to avoid, as much as possible, entering into any minute detail of geological circumstances.

It may however be proper to observe, that the Bovey coal is found in strata, corresponding in almost every particular with those of the surturbrand in Iceland, described by von TROIL,* and by Professor BERGMANN.† The different strata of both these substances are likewise similar, being composed of wood or trunks of trees, which have completely lost their cylindrical form, and are perfectly flattened, as if they had been subjected to an immense degree of pressure.‡

* Von TROIL's Letters, p. 42.

† *Opuscula* BERGMANNI, Tom. III. *De Productis Volcaniis,* p. 239.

‡ BERGMANN, in the dissertation above quoted, accurately describes this appearance of the surturbrand, and then says, "Quæ autem immanis requiritur vis, ut truncus "cylindricus ita complanetur? *Nonne antea particularum nexus putredinis quodam* "*gradu fuerit relaxatus?* Certe, nisi compages quodammodo mutatur, quodlibet "pondus incumbens huic effectui erit impar. Ceterùm idem observatur phænomenon "*in omni schisto argillaceo.*" This is certainly a very curious fact; and the learned Professor, with his usual acuteness, rejects the idea that mere weight can have been the cause. As a farther proof also, he afterwards observes, "Orthoceratitæ, quæ *in strato* "*calcareo conicam figuram perfectè servant,* in schisto *planum fere triangulare com-* "*pressione efficiunt.* Idem valet de piscibus, conchis, insectisque petrefactis." And again, "Observatu quoque dignum est, quod *idem reperiatur effectus, quamvis* "*stratum calcareum sub schisto collocatum sit, et majori ideo pondere comprimente* "*onustum.*" *De Productis Volcaniis,* p. 240. It is evident, therefore, that weight alone has not produced this effect; and BERGMANN's idea, that the solidity of the vegetable bodies may have undergone some previous change, in the manner of incipient putrefaction, by moisture, and by becoming heated in the mass, must be allowed to be very probable. But bodies such as shells could not be thus affected; and therefore they must have been exposed to some mechanical effect, peculiar to argillaceous strata; which effect, however, from the circumstances which have been adduced, evidently could not have resulted from the mere pressure of the superincumbent strata. To me, therefore, it seems not very improbable that, together with a certain change in the

The Bovey coal is commonly of a chocolate-brown, and sometimes almost black. The quality and texture of it are various in different strata; from some of these, it is obtained in the form of straight flat pieces, three or four feet in length, resembling boards, and is therefore called Board Coal. Others have an oblique, wavy, and undulating texture, and, as Dr. MILLES observes, have a strong resemblance to the roots of trees, from which, most probably, this sort has in a great measure been formed.

Some kinds also appear to be more or less intermixed with earth; but that which produces the most powerful and lasting fire, is called stone coal; it is black, with a glossy fracture; has little or none of the vegetable texture; is more solid and compact than the others, being almost as heavy as some of the pit coals, the nature of which it seems very nearly to approach.

For chemical examination, I selected some of the coal which had a wavy texture, and rather a glossy fracture; the quality of this sort being apparently intermediate between the others, as it retains completely the marks of its vegetable origin, while, at the same time, it possesses every perfect character of this species of coal.

solidity of vegetable bodies, produced in the manner imagined by BERGMANN, and, together with some degree of superincumbent pressure, a real and powerful mechanical action has been exerted, by the contraction of the argillaceous strata, in consequence of desiccation; this, I believe, has not hitherto been much considered, but I am inclined, from many circumstances, to attribute to it a very great degree of power.

A. 200 grains of the Bovey coal, by distillation, yielded,	Grains.
1. Water, which soon came over acid, and afterwards turbid, by the mixture of some bitumen - -	60
2. Thick brown oily bitumen - - - -	21
3. Charcoal - - - - - -	90
4. Mixed gas, consisting of hydrogen, carbonated hydrogen, and carbonic acid, } estimated at	29
	200.

The charcoal, in appearance, perfectly resembled that which is made from recent vegetables. By incineration, about 4 grains of yellowish ashes were left, which consisted of alumina, iron, and silica, derived most probably from some small portion of the clay strata which accompany the Bovey coal. But it is very remarkable, that neither the ashes obtained from the charcoal of the Bovey coal, nor those obtained from the leaves of the Iceland schistus, afforded the smallest trace of alkali.*

B. 200 grains of the Bovey coal, reduced to powder, were digested in boiling distilled water, which was afterwards filtrated, and examined; but I could not discover any signs of extract, or of any other substance.

C. 200 grains were next digested with six ounces of alcohol, in a very low degree of heat, during five days. A yellowish-brown tincture was thus formed, which, by evaporation, afforded a deep brown substance, possessing all the properties of resin, being insoluble in water, but soluble in alcohol, and in ether; it

* This, as far as relates to the Bovey coal, has been also noticed by Dr. MILLES. Phil. Trans. Vol. LI. p. 553. But wood, however long submerged, is not deprived of alkali, unless it has more or less been converted into coal; for I have, since the reading of this Paper, made some experiments on the wood of the submerged forest at Sutton, on the coast of Lincolnshire, and have found it to contain potash.

also speedily melted, when placed on a red-hot iron, burned with much flame, and emitted a fragrant odour, totally unlike the very unpleasant smell produced by burning the coal itself, or by burning any of the common bituminous substances. The quantity, however, which could be extracted from 200 grains of the coal, by alcohol, was but small, as it did not exceed 3 grains. But this small quantity was sufficient to prove, that although the Bovey coal does not contain any vegetable extract, like the schistus formerly mentioned, yet the whole of the proximate principles of the original vegetable have not been entirely changed; as a small portion of true resin, not converted into bitumen, still remains inherent in the coal, although the bituminous part is by much the most prevalent, and causes the fetid odour which attends the combustion of this substance.

Upon a comparison of the general external characters of the Bovey coal, with those of the substance which forms the leaves contained in the Iceland schistus, a very great resemblance will be observed; and this is farther confirmed, by the similarity of the products obtained from each of them in the preceding experiments, with the single exception, that the leaves contain some vegetable extract, which I could not discover in the Bovey coal. They agree however in every other respect; as they both consist of woody fibre in a state of semicarbonization, impregnated with bitumen, and a small portion of resin, perfectly similar to that which is contained in many recent vegetable bodies; and thus it seems, that as the woody fibre, in these cases, still retains some part of its vegetable characters, and is but partially and imperfectly converted into coal, so, in like manner, some of the other vegetable principles have only suffered a partial change. Undoubtedly, there is every reason to believe

that, next to the woody fibre, resin is the substance which, in vegetables passing to the fossil state, most powerfully resists any alteration; and that, when this is at length effected, it is more immediately the substance from which bitumen is produced. The instances which have been mentioned corroborate this opinion; for the vegetable extract in one of them, and more especially the resin which was discovered in both, must be regarded as part of those principles of the original vegetables which have remained, after some other portions of the same have been modified into bitumen.

The smallness of the quantity of resin obtained in both the preceding cases, by no means invalidates the proof of the above opinion; but, as an additional confirmation of it, I shall now give an account of a very singular substance, which is found with the Bovey coal.

§ V.

Dr. Milles, in his remarks on the Bovey coal, (which I have several times had occasion to notice in the course of this Paper,) states, that "amongst the clay, but adhering to the coal, are "found lumps of a bright yellow *loam*, extremely light, and so "saturated with petroleum, that they burn like sealing wax, "emitting a very agreeable and aromatic scent."*

This substance, I also observed, when I visited the Bovey coal-pits, in 1794 and 1796. At that time, however, it was scarce, and I could only procure one small specimen, which is now in the British Museum; but, from a cursory examination of it, I was convinced that it was a peculiar bituminous substance, and not loam impregnated with petroleum, as Dr. Milles had supposed. I could not then conveniently make a regular analysis

* Phil. Trans. Vol. LI. p. 536.

of it, and therefore contented myself with briefly describing it, in a note annexed to my Paper on bituminous Substances.*

Lately, however, my friend JOHN SHELDON, Esq. of Exeter, F. R. S. obligingly sent me several pieces of it, together with specimens of the different kinds of Bovey coal which have been mentioned; and thus I was enabled fully to ascertain its real nature and properties.

DESCRIPTION OF THE BITUMEN FROM BOVEY.

It accompanies the Bovey coal, in the manner already described, and is found in masses of a moderate size.

The colour is pale brownish ochraceous yellow.

The fracture is imperfectly conchoidal.

It appears earthy externally, but, when broken, exhibits a slight degree of vitreous lustre.

The fragments are irregularly angular, and completely opaque at the edges.

It is extremely brittle.

It does not apparently become softened, when held for some time in the hand, but emits a faint resinous odour.

The specific gravity, at temperature 65° of FAHRENHEIT, is 1,135.

Some specimens have dark spots, slightly approaching in colour and lustre to asphaltum; and small portions of the Bovey coal are commonly interspersed in the larger masses of this bitumen.

When placed on a heated iron, it immediately melts, smokes much, burns with a bright flame, and yields a very fragrant odour, like some of the sweet-scented resins, but which at last becomes slightly tainted with that of asphaltum.

* Transactions of the LINNEAN Society, Vol. IV. p. 139.

The melted mass, when cold, is black, very brittle, and breaks with a glossy fracture.

EXPERIMENTS.

A. 100 grains of this bitumen, when distilled until the bulb of the retort became red-hot, afforded,

	Grains.
1. Water slightly acid - - - -	3
2. Thick brown oily bitumen, very similar to that which was obtained from the Bovey coal, but possessing slightly the odour of vegetable tar - -	45
3. Light spongy coal - - - -	23
4. Mixed gas, composed of hydrogen, carbonated hydrogen, and carbonic acid, (by computation,) -	29.

The coal yielded about three grains and a half of ashes, which consisted of alumina, iron, and silica, with a trace of lime.

B. The bitumen was not affected by being long digested in boiling distilled water.

C. By digesting 100 grains in lixivium of pure potash, a brown solution was formed; this was saturated with muriatic acid, and a brown resinous precipitate was obtained, which weighed 21 grains.

D. A portion was digested in nitric acid: at first, much nitrous gas was evolved, and, after the digestion had been continued for nearly 48 hours, a part was dissolved, and formed an orange-coloured solution, which did not yield any precipitate, when saturated by the alkalis, or by lime; the colour only became more deep, and, by evaporation, a yellow viscid substance was obtained, which was soluble in water. The above nitric solution possessed every property of those nitric solutions of resinous substances which I have mentioned in a former Paper.*

* Phil. Trans. for 1804, p. 198.

E. The benzoic and succinic acids were not obtained from this substance, by any of the methods usually employed.

F. Alcohol almost immediately began to act upon this bitumen; and, being added at different times, gradually dissolved a considerable part of it. The solution was reddish-brown, and had a resinous odour; by the addition of water it became milky, and, by evaporation, afforded a dark brown substance, which had every property of resin, whilst the residuum left by the alcohol possessed those properties which characterize asphaltum.

The following analysis was then made, to discover the proportions of the component ingredients.

ANALYSIS OF THE BITUMEN FROM BOVEY.

A. 100 grains, reduced to a fine powder, were digested during 48 hours with six ounces of alcohol, the vessel being placed in sand moderately warmed. A deep reddish-brown tincture was thus obtained; and the operation was again twice repeated, with other portions of the same menstruum, until it ceased to act upon the residuum.

The whole of the spirituous solution (which had been cautiously decanted) was then subjected to a very gradual distillation in an alembic, and yielded a brown fragrant resin, which weighed 55 grains.

B. The residuum, which could not be dissolved by alcohol, was digested in boiling distilled water, but this did not act upon it; the whole was therefore collected on a filter, was gradually dried, without heat, by mere exposure to the air, and then weighed 44 grains.

These 44 grains consisted of a light, porous, pale-brown substance, which, being melted, formed a black, shining, brittle mass. It burned with the odour of asphaltum, but rather less

disagreeable, owing most probably to a small portion of the resin, which had not been completely extracted by the alcohol. It was insoluble in water, and in alcohol, but was readily dissolved by heated fat oils; and in every other particular was found to possess the properties of asphaltum.

The 44 grains of asphaltum, when burned, left a residuum, which weighed 3 grains, and consisted of alumina, silica, and iron.

By this analysis it appears, that the bitumen which accompanies the Bovey coal, is a peculiar and hitherto unknown substance, which is partly in the state of vegetable resin, and partly in that of the bitumen called Asphaltum, the resin being in the largest proportion, as 100 grains of the abovementioned substance afforded, -

Resin -	55
Asphaltum -	41
Earthy residuum	3
	99.

Thus we have an instance of a substance being found under circumstances which constitute a fossil, although the characters of it appertain partly to the vegetable, and partly to the mineral kingdom.

§ VI.

The powerful action which alcohol exerts on most of the resins, may justly be regarded as forming a marked distinction between those substances and the bitumens. But, as some of the bitumens are acted upon by alcohol, in a slight degree, I was desirous to ascertain whether a small portion of resin was contained in any of these; or, if that was not the case, I wished to determine the nature of the substance which could be separated, although very sparingly, by this menstruum. I therefore made the following comparative experiments, on the soft brown

elastic bitumen from Derbyshire; on the genuine asphaltum; on very pure cannel coal; and on the common pit coal.

100 grains of each were digested with three ounces of alcohol, in matrasses placed in warm sand, during five days, some alcohol being occasionally added, to supply the loss caused by evaporation. After the abovementioned period had elapsed, the liquid contained in each matrass was poured into separate vessels.

I. The alcohol which had been digested on the elastic bitumen was not tinged, nor, when spontaneously evaporated, did it leave any film or stain on the glass.

II. From asphaltum, the alcohol had extracted a yellow tincture, which, in some situations, appeared of a pale olive colour, and, being spontaneously evaporated, a thick brown liquid was deposited, in small drops, on the glass; these drops did not become hard after two months, and possessed the odour, and every other property, of petroleum. The asphaltum had lost in weight about one grain and a half.

III. The cannel coal had communicated a pale yellow tint to the alcohol, which, in the manner above described, was ascertained to be caused by petroleum; but, from the smallness of the quantity, the weight could not be determined.

IV. The alcohol which had been digested on pit coal, had not assumed any colour; but, by spontaneous evaporation,* it left a film on the glass, which, by its odour, was also found to be petroleum.

By these experiments we find, that the action of alcohol on the bitumens is very slight; and that the small portion which

* Spontaneous evaporation, by exposure to the air, was employed in these experiments, for reasons which must be sufficiently obvious.

may thus be extracted from some of them, is petroleum. In these, the process of bituminization (if I may be allowed to employ such a term) appears to have been completed, whilst in the Bovey coal, and especially in the substance which accompanies it, nature seems to have performed only the half of her work, and, from some unknown cause, to have stopped in the middle of her operations. But, by this circumstance, much light is thrown on the history of bituminous substances; and the opinion, that they owe their origin to the organized kingdoms of nature, especially to that of vegetables, which hitherto has been supported only by presumptive proofs, seems now, in a great measure, to be confirmed, although the causes which operate these changes on vegetable bodies are as yet undiscovered.

Many facts indicate, that time alone does not reduce animal or vegetable bodies to the state of fossils. In this country, there are numerous examples of large quantities of timber, (even whole forests,) which have been submerged prior to any tradition, and which nevertheless completely retain their ligneous characters.* Other local causes and agents must therefore have been required, to form the varieties of coal and other bituminous substances. In some instances, (as in the formation of Bovey coal,) these causes seem to have acted partially and imperfectly, whilst, in the formation of the greater part of the pit coals, their operation has been extensive and complete.

In the pit coals, the mineral characters predominate, and the principal vestige of their real origin seems to be bitumen; for the presence of carbon in the state of oxide, cannot alone be considered as decisive.

* Phil. Trans. for January, 1671. Phil. Trans. Vol. XIX. p. 526. Ibid. Vol. XXII. p. 980. Ibid. Vol. XXIII. p. 1073. Ibid. Vol. XXVII. p. 298. Ibid. for 1799, p. 145.

Bitumen, therefore, with the exuviæ and impressions so commonly found in the accompanying strata, must be more immediately regarded as the proofs, in favour of the origin of pit coal from organized bodies; and, considering the general facts which have been long observed, together with those lately adduced respecting the Bovey coal, and the substance which is found with it, we seem now to have almost unquestionable evidence, that bitumen has essentially been produced by the modification of some of the proximate principles of vegetables, and especially resin.

Modern chemistry had comparatively made but a small progress, when the illustrious BERGMANN published his Dissertation entitled *Producta Ignis subterranei chemice considerata;* for, at that time, the extent and power of chemical action, in the humid way, were very imperfectly understood. In that part, however, of the above work, where he speaks of the fossil wood of Iceland, called Surturbrand, he evidently appears doubtful how far volcanic fire may have acted upon it; although he conceives that, in the formation of it, there has been some connection with volcanic operations. His words are, "Quid de ligno fossili "Islandiæ sentiendum sit, gnaro in loco natali contemplatori "*decidendum relinquimus.* Interea, ut cum vulcani operationibus "nexum credamus, plures suadent rationes, quamvis hucusque "modum ignoremus, quo situm texturamque adquisiverunt "hæc strata." It certainly was very natural that BERGMANN should entertain this opinion, in respect to the surturbrand; and it is remarkable, that the leaves contained in the schistus lately described, are of the same nature, and are found in the same country. The leaves also described by Mr. ST. FOND, are likewise found in a country which, according to him, was formerly volcanic. Were these substances, therefore, never found

but in countries which either actually are or were volcanic, we should be almost compelled to believe, with the Swedish Professor, that the operations of subterraneous fires have been concerned in the formation of these bodies, or rather in the conversion of them into their present state.

But similar substances are found in countries where not the smallest vestige of volcanic effects can be discovered, and Devonshire most undoubtedly is such; yet, nevertheless, the Bovey coal is there found similar to the surturbrand, in most of the external, and (from experiments which I made some years ago, I believe I may say) chemical properties; to which must be added, that both these substances perfectly resemble each other, by forming regular strata.*

Moreover, the half charred appearance of Bovey coal, and of surturbrand, cannot be adduced as any proof, that the original vegetable bodies have been exposed to the partial effects of subterraneous fire; for, at this time, we know that the oxidizement of substances is performed, at least as frequently, and as effectually, by the humid as by the dry way. It would therefore be superfluous here, to enter into an elaborate discussion, to prove that coal and bitumen, with much greater probability, have been formed without the intervention of fire; and I am the less inclined to say more upon this subject, as I have already published some considerations on it in a former Paper.†

Before I conclude, I must beg leave to observe, that as the substance which is found with the Bovey coal is, in every respect, so totally different from any of the bitumens hitherto

* Trans. of the LINNEAN Society, Vol. IV. p. 138. Von TROIL's Letters, p. 42. *Opuscula* BERGMANNI, Tom. III. p. 239.

† Trans. of the LINNEAN Society, Vol. IV. pp. 141, &c.

discovered, it seems proper that it should receive some specific name; and, as it has been proved to consist partly of a resin and partly of a bituminous substance, I am induced to call it *Retinasphaltum*,* a name by which a full definition of its nature is conveyed.

I have lately seen, in No. 85 of the *Journal des Mines*, p. 77, an account of a peculiar combustible fossil, found near Helbra, in the county of Mansfield, and described by Mr. VOIGHT, in his *Versuch einer Geschichte der Steinkohle, der Braunkohle*, &c. p. 188. This substance is of an ash-coloured gray, passing to grayish-white; it is found in a bed of bituminous vegetable earth, which has apparently been produced by the decomposition of fossil wood. The purest specimens are in the form of nodules; the fracture is earthy; it is opaque; soft; brittle; and is very light. When applied to the flame of a candle, it burns and melts like sealing-wax, at the same time diffusing an odour which is not disagreeable. This substance appears to accord in so many properties with the retinasphaltum of Bovey, that I cannot but suspect it to be of a similar nature, and I have little doubt that, by a chemical examination, it will be found to consist partly of resin and partly of bitumen.

* From ῥητίνη, resin; and ἄσφαλτος, bitumen.

XVI. *On two Metals, found in the black Powder remaining after the Solution of Platina. By* Smithson Tennant, *Esq. F. R. S.*

Read June 21, 1804.

Upon making some experiments, last summer, on the black powder which remains after the solution of platina, I observed that it did not, as was generally believed, consist chiefly of plumbago, but contained some unknown metallic ingredients. Intending to repeat my experiments with more attention during the winter, I mentioned the result of them to Sir Joseph Banks, together with my intention of communicating to the Royal Society, my examination of this substance, as soon as it should appear in any degree satisfactory. Two memoirs were afterwards published in France, on the same subject; one of them by M. Descotils, and the other by Messrs. Vauquelin and Fourcroy. M. Descotils chiefly directs his attention to the effects produced by this substance on the solutions of platina. He remarks, that a small portion of it is always taken up by nitro-muriatic acid, during its action on platina; and, principally from the observations he is thence enabled to make, he infers, that it contains a new metal, which, among other properties, has that of giving a deep red colour to the precipitates of platina.

M. Vauquelin attempted a more direct analysis of the substance, and obtained from it the same métal as that discovered by M. Descotils. But neither of these chemists have observed,

that it contains also another metal, different from any hitherto known.

The substance with which my experiments were made, was obtained from platina which had been previously freed from the sand and other impurities generally mixed with it; so that it must have been contained in the substance of the grains of platina. Though it has somewhat the appearance of plumbago, it may easily be distinguished by its superior weight. By weighing it in a phial with water, I found its specific gravity almost 10.7.

Before I describe the method of separating the two metals of which it consists, it may be worth while to mention the effects of it, when combined with different metals in its entire state. It readily unites with lead; but, even with ten times its own weight, the compound has not, when melted, much fluidity. Upon dissolving the lead in nitrous acid, the black powder was obtained, with little apparent alteration, not having been entirely broken down, but consisting chiefly of the same scaly particles as at first. With bismuth, zinc, and tin, the effects were nearly similar; but, by fusion with copper in a very strong heat, a more perfect union was produced. On attempting to dissolve the compound by nitro-muriatic acid, some of the powder was taken up with the copper, forming a very dark solution.

The undissolved portion consisted partly of the substance in its original form of scales, and partly of a blacker powder, the particles of which were too small to be visible, and which had probably been completely combined with the copper. This substance may be easily united, by fusion, with silver or gold; and it is particularly deserving of attention, that it cannot be

separated from these metals, by the usual process of refining. It remains combined with either of them, after cupellation with lead; and with the gold, after quartation with silver. The alloys retain considerable ductility; and the colour of that with gold, is not materially different from pure gold.

I shall now proceed to describe the analysis of the black powder, and the properties of the two metals which enter into its composition. The method which I used for dissolving it, was similar to that employed by M. VAUQUELIN, the alternate action of caustic alkali, and of an acid. I put a quantity of the powder into a crucible of silver, with a large proportion of pure dry soda, and kept it in a red heat for some time. The alkali being then dissolved in water, had acquired a deep orange, or brownish-yellow colour, but much of the powder remained undissolved. This powder, digested in marine acid, gave a dark blue solution, which afterwards became of a dusky olive-green, and finally, by continuing the heat, of a deep red colour. Part of the powder being yet undissolved by the marine acid, was heated as before with alkali; and, by the alternate action of the alkali and acid, the whole appeared capable of solution. At each operation, some silex was taken up by the alkali; and, as this continued till the metallic part was entirely dissolved, it seems to have been chemically combined with it.

The alkaline solution contains the oxide of a volatile metal, not yet noticed, but which I shall presently describe, and also a small proportion of the other metal. If this solution is kept for some weeks, the latter metal separates spontaneously from it, in the form of very thin flakes, of a dark colour.

The acid solution also contains both the metals, but principally that which has been mentioned by the French

chemists. The properties of this last metal, which they have remarked, are those of giving a red colour to the triple salt of platina with sal-ammoniac, of not being altered by muriate of tin, and of giving, with pure alkali, a dark brown precipitate. M. VAUQUELIN also adds, that it is precipitated by galls, and by prussiate of potash; but I should rather ascribe these precipitates to some impurity, and probably to iron.

As it is necessary to give some name to bodies which have not been known before, and most convenient to indicate by it some characteristic property, I should incline to call this metal *Iridium*, from the striking variety of colours which it gives, while dissolving in marine acid.

In order to obtain the compound of this metal with marine acid in a pure state, I tried to make it crystallize.

By slow evaporation of the solution, only an imperfectly crystallized mass was produced; but this, being dried on blotting-paper, and dissolved in water, afforded, by again evaporating as before, distinct octaedral crystals. These crystals, dissolved in water, gave a deep red coloured solution, inclining to orange. With an infusion of galls, no precipitate was formed, but the colour was instantly, and almost intirely, taken away. Muriate of tin, carbonate of soda, and prussiate of potash, produced nearly the same effect. Pure ammonia precipitates the oxide; but (possibly from adding it in excess) I found it retained a part in solution, acquiring a purple colour. The pure fixed alkalis also precipitate the greater part of the oxide, but are capable of retaining a part in solution, becoming of a yellow colour. All the metals which I tried, excepting gold and platina, produced a dark or black precipitate from the muriated solution, which is at the same time deprived of its colour. The

iridium may be obtained in a pure state, merely by exposing the octaedral crystals to heat, which expels the oxygen and the muriatic acid. It appeared of a white colour, and was not capable of being melted, by any degree of heat I could apply. I could not combine it with sulphur, nor with arsenic. Lead easily unites with it; but is separated by cupellation, leaving the iridium upon the cupel, as a coarse black powder. Copper forms with it a very malleable alloy, which, after cupellation with the addition of lead, left a small proportion of the iridium, but much less than in the former case. Silver may be united with it, and the compound remains perfectly malleable. The iridium was not separated from it by cupellation, but occasioned on the surface a dark or tarnished hue. It appeared not to be perfectly combined with the silver, but merely diffused through the substance of it, in the state of a fine powder. Gold alloyed with iridium is not freed from it by cupellation, nor by quartation with silver. The compound was malleable; and did not differ much in colour from pure gold, though the proportion of alloy was very considerable. If the gold or silver is dissolved, the iridium is left, in the form of a black powder.

The yellow alkaline solution, which I have already mentioned as containing a metallic oxide, distinct from the former, is considered by M. VAUQUELIN as a solution of the oxide of chrome in alkali; but I could not, by any test, discover the presence of chrome. After the superfluous alkali had been neutralized by an acid, it produced a pale or buff-coloured precipitate with a solution of lead, and not the bright yellow which is given by chrome. But, as we are indebted to the above distinguished chemist, among many other important discoveries, for our knowledge of the existence of chrome, it is not improbable that

some kinds of platina may contain that substance, besides the other bodies usually mixed with it. When the alkaline solution is first formed, by adding water to the dry alkaline mass in the crucible, a pungent and peculiar smell is immediately perceived. This smell, as I afterwards discovered, arises from the extrication of a very volatile metallic oxide; and, as this smell is one of its most distinguishing characters, I should on that account incline to call the metal *Osmium.*

This oxide may be expelled from the alkali by any acid, and obtained in solution with water by distillation. The sulphuric acid, being the least volatile, is the most proper for this purpose; but as, even of this acid, a little is liable to pass over, a second slow distillation is required, to obtain the oxide perfectly free from it. The solution thus procured is without colour, has a sweetish taste, and the strong smell before mentioned. Paper stained blue with violets, was not changed by it to red; but, by being exposed to the vapour of it in a phial, the paper lost much of its blue colour, and inclined to gray. As a certain quantity of this oxide is extricated during the solution of the iridium in marine acid, that part may also be obtained by distillation.

Another mode by which the oxide of osmium may be obtained in small quantity, but in a more concentrated state, is, by distilling with nitre the original black powder procured from platina.

With a degree of heat hardly red, there sublimes into the neck of the retort, a fluid apparently oily, but which, on cooling, concretes into a solid, colourless, semitransparent mass. This, being dissolved in water, forms a solution similar to that before described. The oxide, in this concentrated state, stains the skin of a dark colour, which cannot be effaced. The most striking

test of the oxide of osmium, is an infusion of galls, which presently produces a purple colour, becoming soon after of a deep vivid blue. By this means, the presence of this, and of the metal first described, may be observed, when the two are mixed together. The solution of iridium is not apparently altered by being mixed with the oxide of osmium; but, on adding an infusion of galls, the red colour of the first is instantly taken away, and soon after the purple and blue colour of the latter appears. The solution of the oxide of osmium with pure ammonia, becomes somewhat yellow, and slightly so with carbonate of soda. It is not affected by pure magnesia, nor by chalk; but with lime a solution is formed, of a bright yellow colour. The solution with lime gives with galls a deep red precipitate, which becomes blue by acids. It produces no effect on a solution of platina or gold; but precipitates lead of a yellowish-brown, mercury of a white, and muriate of tin of a brown colour.

The oxide of osmium becomes of a dark colour with alcohol, and, after some time, separates in the form of black films, leaving the alcohol without colour. The same effect is produced by ether, and much more quickly.

This oxide appears to part with its oxygen to all the metals, excepting gold and platina. Silver being kept in a solution of it for some time, acquires a black colour; but does not entirely deprive it of smell. Copper, tin, zinc, and phosphorus, quickly produce a black or gray powder, and deprive the solution of all smell, and of the power of turning galls of a blue colour. This black powder, which consists of the osmium in a metallic state and the oxide of the metal employed to precipitate it, may be dissolved in nitro-muriatic acid, and then becomes blue with infusion of galls.

If the pure oxide of osmium, dissolved in water, is shaken with mercury, it very soon loses its smell; and the metal, combining with the mercury, forms a perfect amalgam.

Much of the mercury may be separated by squeezing it through leather, which retains the amalgam of a firmer consistence. The remaining mercury being distilled off, a powder is left, of a dark gray or blue colour, which is the osmium in its pure state. By exposing it to heat with access of air, it evaporates, with the usual smell; but, if the oxidation is carefully prevented, it does not seem in any degree volatile. Being subjected to a strong white heat, in a cavity made in a piece of charcoal, it was not melted, nor did it undergo any apparent alteration. Heated in a similar situation with copper and with gold, it melted with each of these metals, forming alloys which were quite malleable. These compounds were easily dissolved in nitro-muriatic acid, and, by distillation, afforded the oxide of osmium with the usual properties.

The pure metal which has been previously heated, does not seem to be acted on by acids; at least I could not perceive any effect produced by boiling it for some time with nitro-muriatic acid. By heating it in a silver cup with caustic alkali, it immediately combined with the alkali, and, with water, gave a yellow solution, similar to that from which it was procured. Acids expel from this solution the oxide of osmium, which has the usual smell, and the power of giving to infusion of galls the blue colour before mentioned.

XVII. *On a new Metal, found in crude Platina.* *By* William Hyde Wollaston, *M. D. F. R. S.*

Read June 24, 1804.

NOTWITHSTANDING I was aware that M. DESCOTILS had ascribed the red colour of certain precipitates and salts of platina, to the presence of a new metal; and although Mr. TENNANT had obligingly communicated to me his discovery of the same substance, as well as of a second new metal, in the shining powder that remains undissolved from the ore of platina; yet I was led to suppose that the more soluble parts of this mineral might be deserving of further examination, as the fluid which remains after the precipitation of platina by sal ammoniac, presents appearances which I could not ascribe to either of those bodies, or to any other known substance.

My inquiries having terminated more successfully than I had expected, I design in the present Memoir to prove the existence, and to examine the properties, of another metal, hitherto unknown, which may not improperly be distinguished by the name of *Rhodium*, from the rose-colour of a dilute solution of the salts containing it.

I shall also take the same opportunity of stating the result of various experiments, which have convinced me, that the metallic substance which was last year offered for sale by the name of Palladium, is contained (though in very small proportion) in the ore of platina.

The colour of the solution that remains after the precipitation of platina, varies, not only according to its state of dilution, but also according to the strength and proportions of the nitric and muriatic acids employed. This colour, though principally owing to the quantity of iron contained in it, arises also in part from a small quantity of the ammoniaco-muriate of platina, that necessarily remains dissolved, and from other metals contained in still smaller proportions.

(A. 1.) To recover the remaining platina, as well as to separate the other metals that are present from the iron, I have in some experiments employed zinc, in others iron, for their precipitation. The former appears preferable; but, when the latter has been used, the precipitate may immediately be freed from the iron that adheres to it, by muriatic acid, without the loss of any of those metals which are at present the subject of inquiry.

(A 2.) Having in one instance dissolved such a precipitate in nitro-muriatic acid, and precipitated the platina by sal ammoniac, I suffered the remaining fluid to evaporate without heat; and obtained a mixture of various crystals, very different from each other in form and colour. From these, I selected for examination some that were of a deep red colour, partly in thin plates adhering to the sides of the vessel, and partly in the form of square prisms having a rectangular termination.

(A 3.) A portion of these crystals being heated in a small tube, yielded sal ammoniac by sublimation, and left a black residuum, which, by greater heat, acquired a brilliant metallic whiteness, but could not be fused under the blowpipe. Having obtained this substance from a distinctly crystallized salt, I was inclined to consider it as a simple metal; and, as I found it to

be wholly insoluble in nitro-muriatic acid, I judged it not to be platina.

(A 4.) The crystals also, instead of being nearly insoluble, like the ammoniaco-muriate of platina, were dissolved in a small quantity of water, and gave a rose-coloured solution. Upon mixing this with a solution of platina, the ammonia was transferred by superior affinity to the latter, forming an ammoniaco-muriate of platina; and the precipitate was of a yellow colour. Consequently, the metal contained in the salt, was neither platina nor that which gives the red colour to the salts of platina.

It would be useless to detail my first unsuccessful experiments, made upon the properties of this metal, in hopes of discovering means by which its separation from platina might be effected; I shall therefore confine myself to the following process, which appears to be the most direct for procuring rhodium in a state of purity. In the same process also palladium is obtained, so as to afford a presumption, that it is rather a natural simple body, than any artificial compound.

(B 1.) Since the platina to be procured in this country, generally contains small scales of gold intermixed, as well as a portion of the mercury which the Spaniards employ for the separation of the gold, the platina used for my experiments, after being by mechanical means freed, as far as possible, from all visible impurities, was exposed to a red heat, for the purpose of expelling the mercury. It was then digested for some time in a small quantity of dilute nitro-muriatic acid, and frequently shaken, till the whole of the gold was dissolved, together with any impurities that might superficially adhere to the grains of platina.

(B 2.) Of the ore thus prepared, nearly 2½ ounces were then dissolved in nitro-muriatic acid, (diluted for the purpose of leaving as much as possible of the shining powder,) and the whole suffered to remain in a moderate sand heat, till completely saturated.

(B 3.) Such a portion of this solution was then taken for analysis, as corresponded to 1000 grains of the prepared ore. An ounce of sal ammoniac was next dissolved in hot water, and used for the precipitation of the platina. The precipitate obtained was of a yellow colour, and, upon being heated, yielded 815 grains of purified platina.

(B 4.) The water used for washing this precipitate having been added to the solution poured from it, a piece of clean zinc was immersed in it, and suffered to remain, till there appeared to be no further action upon the zinc. The iron contained in the ore (to the amount of 14 or 15 *per cent.)* remained in solution. The other metals had subsided, in the form of a black powder, which I estimated between 40 and 50 grains; but, as there was no occasion to weigh it with accuracy, I thought it better not to dry this precipitate, for, if it be heated, the rhodium is in danger of being rendered insoluble.

(B 5.) As I had previously ascertained that this precipitate would contain platina, rhodium, the substance called palladium, copper, and lead, the two last metals were first dissolved in very dilute nitric acid, aided by a gentle heat. The remainder, after being washed, was digested in dilute nitro-muriatic acid, which dissolved the greater part, but left as much as 4½ grains undissolved.*

* It was presumed that this residuum consisted principally of the metal called by Mr. TENNANT Iridium; but, as it was accidentally mislaid, and was not examined, it might also contain a portion of rhodium.

(B 6.) To the solution were added 20 grains of common salt; and, when the whole had been evaporated to dryness with a very gentle heat, the residuum, which I had found, from prior experiments, would consist of the soda-muriates of platina, of palladium, and of rhodium, was washed repeatedly with small quantities of alcohol, till it came off nearly colourless. There remained a triple salt of rhodium, which by these means is freed from all metallic impurities.

(C 1.) This salt, having been dissolved in a small quantity of hot water, and let to stand 12 hours, formed rhomboidal crystals, of which the acute angle was about 75°.

(C 2.) It was then again dissolved in water, and divided into two equal portions. Of these, one was decomposed by a piece of zinc, and the other examined by the following reagents.

(C 3.) Sal ammoniac occasioned no precipitation; but, when a solution of platina was added to the mixture, a precipitate was immediately formed, and the colour of this precipitate was yellow; which again proves that the metal contained in this salt, is neither platina itself nor that which gives the red colour to its precipitates.

(C 4.) Prussiate of potash occasioned no precipitation, as it would have done, if the solution had contained palladium.

(C 5.) Hydro-sulphuret of ammonia, which would have precipitated either platina or palladium, caused no precipitation of this metal.

(C 6.) The carbonates of potash, of soda, or of ammonia, occasioned no precipitation; but the pure alkalis precipitated a yellow oxide, soluble by excess of alkali, and also soluble in every acid that I have tried.

(D 1.) The solution of this oxide in muriatic acid, upon being

evaporated, did not crystallize; the residuum was soluble in alcohol, and of a rose colour. Sal ammoniac, nitre, or common salt, caused no precipitation from the muriatic solution; but formed triple salts, which were not soluble in alcohol.

(D 2.) The solution in nitric acid also did not crystallize. A drop of this solution, being placed upon pure silver, occasioned no stain. On the surface of mercury a metallic film was precipitated, but did not appear to amalgamate. The metal was also precipitated by copper and other metals, as might be presumed, from the usual order of their affinities for acids.

(E 1.) The precipitate obtained by zinc (C 2.) from the remaining half of the salt, appeared in the form of a black powder, weighing, when thoroughly dried, nearly 2 grains, corresponding to about 4 grains in the 1000 of ore dissolved.

(E 2.) When exposed to heat, this powder continued black; with borax, it acquired a white metallic lustre, but appeared infusible by any degree of heat.

(E 3.) With arsenic, however, it is, like platina, rendered fusible; and, like palladium, it may also be fused by means of sulphur. The arsenic, or the sulphur, may be expelled from it by a continuance of the heat; but the metallic button obtained does not become malleable, as either of the preceding metals would be rendered by similar treatment.

(E 4.) It unites readily with all metals that have been tried, excepting mercury; and, with gold or silver it forms very malleable alloys, that are not oxidated by a high degree of heat, but become incrusted with a black oxide, when very slowly cooled.

(E 5.) When 4 parts of gold are united with 1 of rhodium, although the alloy may assume a rounded form under the

blowpipe, yet it seems to be more in the state of an amalgam than in complete fusion.

(E 6.) When 6 parts of gold are alloyed with 1 of rhodium, the compound may be perfectly fused, but requires far more heat than fine gold. There is no circumstance in which rhodium differs more from platina, than in the colour of this alloy, which might be taken for fine gold, by any one who is not very much accustomed to discriminate the different qualities of gold. On the contrary, the colour of an alloy containing the same proportion of platina, differs but little from that of platina. This was originally observed by Dr. Lewis. "The colour was still so "dull and pale, that the compound (5 to 1) could scarcely be "judged by the eye to contain any gold."*

I find that palladium resembles platina, in this property of destroying the colour of a large quantity of gold. When 1 part of palladium is united to 6 of gold, the alloy is nearly white.

(E 7.) When I endeavoured to dissolve an alloy of silver or of gold with rhodium, the rhodium remained untouched by either nitric or nitro-muriatic acids; and, when rhodium had been fused with arsenic or with sulphur, or when merely heated by itself, it was reduced to the same state of insolubility. But, when 1 part of rhodium had been fused with 3 parts of bismuth, of copper, or of lead, each of these alloys could be dissolved completely, in a mixture of 2 parts, by measure, of muriatic acid, with 1 of nitric. With the two former metals, the proportion of the acids to each other seemed not to be of so much consequence as with lead; but the lead appeared on another account preferable, as it was most easily separated, when reduced to an insoluble muriate by evaporation. The muriate of rhodium had then the same colour

* Lewis's Philosophical Commerce of Arts, p. 526.

and properties, as when formed from the yellow oxide precipitated from the original salt. (D 1.)

(E 8.) The specific gravity of rhodium, as far as could be ascertained by trial on so small quantities, seemed to exceed 11. That of an alloy consisting of 1 part rhodium and about 2 parts lead, was 11,3; which is so nearly that of lead itself, that each part of this compound may be considered as having about the same specific gravity.

F. As it was expected that the alcohol employed for washing the salt of rhodium (B 6.) would contain the soda-muriates of platina and of palladium, the platina was first precipitated by sal ammoniac. This precipitate was of a deep red colour; and, when it had been heated, to expel the sal ammoniac, the platina which remained was of a dark gray colour.

(G 1.) To the remaining solution, after it had been diluted to prevent any further precipitation of platina, I added prussiate of potash, which instantly occasioned a very copious precipitate, of a deep orange-colour at first, but changing afterwards to a dirty bottle-green, which I ascribed to iron contained in the prussiate.

(G 2.) This precipitate, when dry, weighed $12\frac{1}{2}$ grains. After it had been heated, it left a metallic residuum, in small grains, of a gray colour, weighing nearly 7 grains. A small portion of it being heated with borax, communicated a dark brown colour to the borax, as from iron, and acquired a bright metallic lustre, but could not be fused under the blowpipe. With sulphur, however, it fused immediately into a round globule, which, by floating upon mercury, appeared of less specific gravity than that metal.

(G 3.) The whole quantity was then treated in the same manner, and purified by cupellation with borax, till it cooled with a bright surface. From the globule the sulphur was expelled, by exposure to the extremity of the flame; and it became spongy and malleable, weighing in this state very nearly 5 grains.

(G 4.) A portion of this metal was dissolved in strong nitrous acid, was precipitated by green sulphate of iron, and, in other respects, possessed all the properties ascribed to the palladium offered for sale, in the printed paper that accompanied it, as well as others since noticed by Mr. Chenevix.

(G 5.) In its precipitation by prussiates, it differs most essentially from platina; and consequently is by no means difficult to be distinguished, or separated from it.

(G 6.) The action of muriate of tin upon the solutions of these metals, is also totally different. A dilute solution of platina, is thereby changed from a pale yellow to a transparent blood-red. A solution of palladium, on the contrary, usually becomes opaque, by the formation of a brown or black precipitate; but, if mixed in such proportion as to remain transparent, it changes to a beautiful emerald-green.

(G 7.) In the formation of triple salts with the alkalis, as observed by Mr. Chenevix, palladium may be said to resemble platina; but the salts thus formed are far more soluble than the corresponding salts of platina, and differ entirely, in the colour and form of the crystals.

(G 8.) The soda-muriate of palladium is a deliquescent salt; that of platina, on the contrary, forms permanent crystals.

(G 9.) The triple salts of platina, with either muriate of ammonia or of potash, form octaedral crystals of a yellow

colour, that are very sparingly soluble in water. The corresponding salts of palladium, likewise resemble each other in every respect. The crystals are very soluble in water, but insoluble in alcohol; their form is that of a four-sided prism, and they each present a curious contrast of colour, that certainly is not observable in any known salt of platina.

(G 10.) Although the solution is of a deep red, the crystals are of a bright green when viewed transversely. In the direction of their axes, however, the colour is the same as that of the solution; but, on account of its extreme intensity, it is with difficulty distinguished in fragments that exceed $\frac{1}{100}$ of an inch in thickness. One consequence of this colour is nevertheless very observable; namely, that in viewing any crystal obliquely, it appears of a dull brown, that arises from a mixture of the red and green.*

The characters of palladium that have been enumerated, undoubtedly belong to none of the simple substances that we are

* The change of colour above described, though certainly uncommon, is nevertheless not peculiar to the salts of palladium, but may be seen also in some kinds of tourmalin. Among those which come to us from Ceylon, some are transparent; and one variety is of a deep red in the direction of its axis, but of a yellowish-green when viewed transversely. There is also a corresponding, but opposite contrast of colours, that has been observed by MÜLLER, and described by BERGMANN, in some of the Tyrolese tourmalins. The general aspect of these stones is black, and apparently opaque. Some, however, of which the fracture is vitreous, are found to transmit a yellowish-red light when viewed transversely, but in the direction of their axis the colour is a dull bottle-green.

In each of these tourmalins, as well as in the salts of palladium, the colour in the direction of the axis, is at least 10 times more intense than in the transverse direction. A thin lamina, cut from the end of a Tyrolese tourmalin for this purpose, transmitted no visible light, till it was reduced to $\frac{1}{60}$ of an inch in thickness; and, when less than $\frac{1}{100}$ of an inch, it was not more transparent than another portion of the same crystal seen transversely, $\frac{1}{10}$ of an inch in thickness.

acquainted with; and no experiment that I have made, has tended to confirm the suspicion of its being a compound, consisting of any known ingredients. The experiments above related, show evidently, that the ore of platina contains a very small quantity of palladium; and it is not unlikely that this may have been a constituent part of some of the compounds obtained by Mr. CHENEVIX, and may have misled him, by some properties which he would consequently observe, into the supposition that he had formed palladium.

It is not, however, without having repeatedly endeavoured to imitate his experiments, that I have ventured to dissent from such authority. I made many attempts to unite pure platina with mercury, by solution, and by amalgamation; but without success, in any one instance.

From a solution of platina, carefully neutralized, as Mr. CHENEVIX directs, with red oxide of mercury, and mixed with a solution of green sulphate of iron, I indeed obtained such a precipitate of metallic flakes as he describes; but, upon examination of these flakes, they yielded mercury by distillation; and the remainder consisted of platina combined with a portion of iron, but had not any properties which I could suppose owing to the presence of palladium.

Upon comparing the specific gravity of this substance, which was said to be, at most, 11,8, with that of mercury or of platina, I was always strongly inclined to doubt the possibility of its being composed of these metals. I could recollect no one instance, in which the specific gravity of any compound is less than that of its lightest ingredient, and could not, without careful examination, admit the supposition, that mercury could be rendered lighter by intimate union with platina. It now appears

fully confirmed that this persuasion, arising from uniform experience, was well founded; for, if we consider the difficulty of producing even an imperfect imitation of palladium, the failure of all attempts to resolve it into any known metals, the facility of separating it from any mixed solution of those which it has been supposed to contain, as well as the number and distinctness of its characteristic properties, I think we must class it with those bodies which we have most reason to consider as simple metals.

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